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Psychological Bulletin

PARTIAL REINFORCEMENT: A REVIEW AND CRITIQUE

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I. Introduction

Little more than a decade has elapsed since the majority of psychologists interested in learning theory first became aware that there are important differences between continuous and non-continuous reinforcement, particularly when resistance to experimental extinction is used as a measure of response strength. During the 11-year period from 1939 through 1949 at least 30 experimental studies were reported in which the latter kind of reinforcement played a prominent role. Various animals, including humans, were reinforced on some but less than 100% of a series of trials or responses in situations where many kinds of behavior were measured. The very recency of these experiments and their diversity have resulted in a considerable body of uncoordinated data that needs to be analyzed critically and presented in systematic form to serve both as a current summary and a guide to further research.

Although "partial" is in certain respects not a thoroughly suitable adjective for describing the kind of reinforcement with which this paper deals, it has been widely used to indicate various combinations of frequency and pattern, parameters that can be manipulated independently. "Intermittent" and "discontinuous," two other words that sometimes take its place, both connote periodicity, while "partial" will accommodate the important random pattern with ease. "Continuous reinforcement" is used to cover the 100% case, "regular" implying periodic and most commonly employed. As discussed in this paper and the studies upon which it is based the term "*partial reinforcement*"

[1] This research was facilitated by the Laboratory of Social Relations, Harvard University.

refers to reinforcement given at least once but omitted on one or more of the trials or after one or more of the responses in a series. Prolonged non-reinforcement following a minimum of a single reinforcement constitutes the conditions necessary to produce experimental extinction and is the limiting case of partial reinforcement. While partial reinforcement usually refers to primary reward and the emphasis is on this concept in the present paper, the term as here used is intended to cover the secondary or derived reward situation as well.

The experimental design most often used by psychologists involves all-or-none reinforcement—100% (conditioning) or 0% (extinction). In this situation every "correct" response is always reinforced during acquisition and never reinforced during extinction. For a long while response strength appeared to be proportional only to the number of reinforcements without consideration of the percentage of correct responses rewarded or the pattern of reinforced and non-reinforced responses, although the relationship was not thought to be linear (~~26, 43, 51, 70~~). Experimenters assumed that fewer reinforcements (and, implicitly, even if interspersed with non-reinforcements) would produce less response strength than a larger number. Later evidence has not substantiated this implicit postulate.

Early studies. Here and there during the first third of the 20th century we find indications that an awareness of the importance of partial reinforcement was dawning, though somewhat hazily and fortuitously. Razran (54) cites a 1912 experiment performed by Platonov (53) in which, after the CR had been established, the US was applied only on the first trial of each day and yet the CR was maintained. He also comments on the need for "optimal distributions" of reinforcements, stating that many experimenters do not realize the inefficiency of continuous reinforcement in maintaining a CR.

Pavlov (50) explicitly recognized the importance of partial reinforcement and emphasized that it seemed to involve "some further condition which has up to the present been overlooked" (p. 386). In the next-to-last chapter of *Conditioned Reflexes* he disclosed for the first time exploratory experimentation conducted on one dog in which three different patterns of partial reinforcement were used. "The first agent to which conditioned properties were to be given was applied alternately with and without reinforcement by food" (p. 384). This produced conditioning by the 20th application, about as quickly as was customary with the continuous method. Following the CS with food on every third appearance of the former effected conditioning in almost as short a time (by the seventh application) as would ordinarily be obtained with continuous reinforcement under similar conditions, but the dog became quite "excited." Presenting food after each fourth presentation of the

CS during 240 repetitions (a total of 60 presentations of the reinforcing stimulus) did not, however, result in conditioning.

Wolfle (**71**), in a minor phase of her study of intervals separating the CS and the US, found that omitting the 10 test trials per day for the first three days in one group (200 pairs of stimuli per day) resulted on the fourth and fifth days in a slightly larger percentage of CRs in that group than in the others, though the differences were small and not statistically significant. Nevertheless, she concluded that "the lack of conditioning with practice was caused in part by the number of unreinforced conditioned stimuli presented (the phenomenon of experimental extinction)" (p. 101).

As early as 1933 Skinner (**60**) conceived the idea of "periodically reconditioning" animals by giving reinforcement for a response only after a definite interval of time had elapsed, regardless of what activity took place during the interval between reinforcements. This technique, which he called "periodic reconditioning" (and later "periodic reinforcement"), formed the basis for the earliest systematic investigations of partial reinforcement. As a logical sequel of reinforcement with respect to time, Skinner (**61**) studied behavior as a function of reward per number of responses, calling this technique "reinforcement at a fixed ratio."

During the early 1930's Egon Brunswik at the University of Vienna was concerned with "equivocality . . . in the environmental causal couplings" (**68**, p. 45) an interest that later resulted in his probability theory of discrimination (**3**, **4**). In a 1935 article written with Tolman (**68**) the importance of such lack of unequivocality in "means-end relationships" was stressed.

Several years later Tolman (**67**) remarked that "Brunswik has recently brought to light a new point in our ignorance. He has been trying the effect of rewarding on the right and rewarding on the left different proportions of times. In other words, it was no God-given rule but apparently some merely human predilection on our part which made us heretofore tend almost invariably to make one of the alternate behaviors always rewarded and the other always punished. But other frequencies of reward and punishment are equally possible and equally deserving of study" (pp. 5–6).

A wave of partial reinforcement studies appeared in the literature around 1939–40 (**1**, **3**, **6**, **16**, **28**, **29**, **30**, **31**, **38**). Involving many kinds of behavior—eyeblink, leg flexion, salivation, verbal responses, GSR, and so forth—they dealt with the effects of this variable on acquisition, maintenance of behavior, and resistance to extinction.

Importance of partial reinforcement. The theoretical and practical significance of partial reinforcement cannot be overestimated. Any comprehensive system of behavioral principles must account for the fact that behavior can be established and maintained with reward occurring only a portion of the time. The findings regarding resistance

to extinction after partial as contrasted with continuous reinforcement must be included in this treatment.

Partial reinforcement seems to be a basic condition of everyday behaving, for the rare instance is the presentation of reward each time a response occurs. (In this context reward is used to cover both the primary and secondary situations.) Examples, somewhat oversimplified, are easy to find: The animal trainer feeds his dogs only after several occurrences of the desired act, the clerk receives his check only once each month, the psychologist publishes his findings only after prolonged endeavor, the gambler continues to bet despite an infrequent payoff, *ad infinitum*. Continuous reinforcement seems to be the exception, not the rule.

Both practically and experimentally, partial reinforcement is a worthwhile procedure. It retards satiation effects and permits continued training over long periods and large numbers of trials. Accomplished animal trainers rarely feed their subjects after the completion of a single phase of an act, having hit on the efficiency of the method so that weeks of training can be condensed into a few days.

In brief, partial reinforcement is such a ubiquitous phenomenon that one wonders why psychologists for so long failed to examine its consequences.

The breakdown of the present paper is as follows. First the various techniques for presenting partial reinforcement are discussed. Included in this methodology section are comments and criticisms of partial reinforcement experiments, regarding problems in design and variable control. The four major sections of the paper deal with acquisition, post-learning performance, extinction and theoretical considerations. Some of the practical implications of partial reinforcement are indicated.

II. Questions of Design and Methodology

There are certain methodological considerations, not conveniently discussed in connection with the later sections of this paper dealing with acquisition, performance, and extinction, that are of sufficient generality and importance to warrant separate treatment. A number of these are discussed in the following paragraphs.

Techniques of presenting partial reinforcement. The two major parameters in varying schedules of reinforcement are *frequency* and *pattern*. The distinction between them has sometimes been ignored and the two characteristics have been allowed to vary together, although the problem of patterning is present whenever frequency is less than 100% and greater than 0%. Mowrer and Jones (47) have pointed out that on several occasions experimenters have compared results from one group rewarded continuously and regularly and another rewarded discontinuously and irregularly.

With regard to frequency the whole gamut may be run from continuous

reinforcement to reinforcing not at all (extinction). In practice, the working limits have been 75%–80% (**1, 3, 17, 66**) to 1% or less (**21, 34, 35, 36, 41, 62, 63**). Regardless of frequency, pattern may be regular or irregular.

Another way of considering methods of presenting partial reinforcement is to refer frequency and pattern to the continua on which they are varied in the experimental situation. These points of reference are *time* and *number of responses*. The reinforcing stimulus may be presented according to a regular or an irregular schedule in time at a high or low frequency. Similarly, frequency of reward may be high or low in terms of number of responses, with the pattern either regular or irregular. The most systematically studied case of regular reinforcement in time is Skinner's (**62**) periodic reinforcement.

The irregular time schedule, known as aperiodic reinforcement, has been recently developed (**63**) and employed (**14, 35, 36, 41**). By this method a rat in the bar-pressing apparatus might receive reinforcement for a response on the average of once every five minutes, with a range from one to nine minutes and a step unit of one minute. The intervals are presented in a randomized order. An asset of this procedure is that it precludes the decline in strength found with prolonged periodic reinforcement. The organism is given no basis for discriminating when reinforcements are, or are not, to be presented in time as in the periodic technique (**62**).

Presenting reinforcement for a regular number of responses has been called "reinforcement at a fixed ratio" by Skinner (**62**). In this situation the subject is given the reward only after it has accomplished a certain number of units of the behavior required, e.g., 50 bar-pressings. Skinner (**62**) has varied frequency from a reward for 16 responses to one in 192 bar-pressings.

The aperiodic counterpart in number of responses may be called random or variable ratio reinforcement. The organism might be rewarded on the average of once in 50 responses, with a range from one in 10 to one in 90 and a step interval of 10. This technique has been widely used with apparatus such as the runway and the T-maze (**1, 12, 13, 28, 29, 30, 32, 47, 59**).

A variable confounding the administration of partial reinforcement schedules is the type of experimental set-up employed. The situations that need to be distinguished are (1) those in which neither the experimenter nor the environment restricts the responding, e.g., bar-pressing. Throughout this paper we refer to this instance as "free responding." (2) A different situation is one where opportunities for responding are controlled by the experimenter, i.e., where trials are involved. The latter subsumes single-response instances such as the straight runway and alternative-response (T-unit) or multiple-response set-ups, e.g., various combinations of lights providing the stimulus patterns for several differential responses. The free-responding situation may be converted to the trial instance by withdrawal of the critical aspect of the environment (e.g., the bar) after each response (**52**).

One of the major differences between the free-responding and trial situations lies in the presentation of partial reinforcement according to a time schedule. The latter loses much of its meaning where the set-up is broken down into discrete trials. Where the time continuum is the basis for determining trials (viz., **50**, p. 41, and **2**), however, variations of reinforcement in time apply directly. Other differences lie in frequency and pattern. Typically, a randomized schedule of 50% reinforcement has been employed in the trial situation. Considerably lower frequencies administered regularly or irregularly have been used in the

free-responding set-up. Methods of presenting partial reinforcement in the two situations are summarized in Table I.

TABLE I

TECHNIQUES OF PRESENTING PARTIAL REINFORCEMENT

Variations of Reinforcement	Situation			
	Free-Responding	*Trials*		
		Single Response	Alternative Responses	Multiple Responses
Time				
Regular	Skinner Periodic reinforcement	Time variations not ordinarily used		
Irregular	(36) Aperiodic reinforcement			
Number of Responses				
Regular	Skinner Fixed ratio reinforcement	Fixed ratio reinforcement		
Irregular	Random ratio reinforcement	Mowrer & Jones Random ratio reinforcement		

Sequence of experimental conditions. One of the basic dilemmas facing the investigator of partial reinforcement is that of getting behavior started under a partial schedule. For example, it is clearly impractical to try to start rats from scratch on a 10-minute periodic schedule in the bar-pressing situation. Even with exceptionally high drive it is quite probable that because of the infrequency of reinforcement little or no conditioning will result. On the other hand, if continuous reinforcement is employed to condition the behavior, pure partial reinforcement in conditioning does not occur. Clearly, behavior must attain a minimal level of strength before anything can be done; continuous reinforcement is usually employed.

In a unique experiment Keller (38) has attacked this problem. After receiving enough continuous reinforcements to insure adequate strength of conditioning, one group of rats in the bar-pressing apparatus went through a sequence of continuous followed by periodic reinforcement. The other group had the conditions reversed. As measured by resistance to extinction there was a significant superiority of the group with continuous reinforcement just prior to extinction, but this difference held up for only the first five minutes of extinction. (Another recent study (9) supports this finding; see Section V.) Responding in the remaining 175 minutes of extinction was not significantly different for the two squads.

From these data it appears that the sequence of operations, typically continuous followed by partial reinforcement, may introduce a constant error if the

extinction period is truncated. Otherwise, it seems unlikely that this variable will be critical in the free-responding situation.

In experimental instances involving trials, several experimenters (3, 7, 12, 13) have been able to start their subjects off with a partial schedule. It should be noted that in these studies the frequency of reinforcement is usually high, of the order of 50%, and that ordinarily some form of preliminary training has been given.

In several studies such unusual sequences of experimental conditions have been employed that it seems advisable to discuss them as methodological points. One of these is Humphreys' (29) study of "expectations" in which he trained 78 students under 100% "reinforcement," extinguished the response, and then used the *same* subjects immediately as a 50% reward group with conditions otherwise unchanged.[2]

Brogden (1) reinforced three groups of four dogs each 80%, 60%, 40%, and 20% of the trials. There are 24 permutations of four things taken four at a time, but Brogden was forced to discard 20 when deciding upon the order of presenting the various frequencies, the order being the same in each group. The permutations he chose were systematically biased, since in nine of the 12 cases a less frequent occurrence of reward followed a more frequent one. The general downward trend in frequency of reinforcement (plus the large number of trials at each level) may well have produced an adaptation or general learning effect to maintain the response at a high level with a low frequency of reinforcement.

In an early partial reinforcement experiment, Cole (6) conditioned 39 college students (US, shock; CS, red light; CR, eyeblink) and then extinguished the CR, after which he presented the US on alternate trials in one-half the group and gave one reinforced trial followed by three non-reinforced trials in the other half. As in Humphreys' (29) study, some carry-over effect from the first conditioning and extinction is to be expected.

Murphy (49) used a much more complicated design with adult subjects, first giving them considerable practice on a modified bagatelle (pinball) machine and then successively providing 100% "reinforcement" in the form of a flash of light, extinction, 100% reinforcement with the plunger at another setting, extinction, and two series of 20%, 10% and 5% fixed-ratio reinforcement, each pattern being followed by extinction.

As in Brogden's (1) study, the general sequence and the possible interactions and general learning effects as the reward progressed from 100% to 5% may possibly have acted to maintain the response at a high level even with infrequent reinforcement. The effect of the interpolated extinctions cannot be evaluated directly. Another point to be made is that the frequency of responding per minute in preliminary sessions was slightly *lower* when the reinforcement (light) was used than when it was not.

It is noteworthy that sequential effects have not been explored systematically on a large scale. What is needed is an investigation of a progressive increase in frequency of reinforcement from, say, 10% to 100% by steps of 10% and study of the consequences of the reverse sequence.

It is difficult to evaluate the influence of these sequential variables without

[2] Apparently this fact has remained unnoticed. Hilgard (23) mentions "half the subjects" (p. 273) and refers to "one group" and "the second group" (p. 275).

additional experimentation. Progressive effects may well have been operating in several of the instances cited to produce considerably different results than would have been obtained with independent or matched groups.

Inter-trial interval. In addition to the usual consequences of massing or spacing practice there are two phases of the inter-trial interval that have special effects for partial reinforcement studies. Sheffield (**59**) has recently demonstrated that the spacing variable is critical in the superior resistance to extinction found after partial reinforcement. Most studies have involved massed training. Secondly, Heathers' (**20**) findings of "spontaneous alternation," recently extended by Stanley (**66**), are relevant to T-maze investigations. This factor is not crucial, but operates to retard learning if trials are massed in dual-response situations.

Number of trials and number of reinforcements. One general dilemma characterizes the partial reinforcement situation: the relationship between number of trials (or responses) and number of reinforcements. A partial reinforcement group can be matched with a continuously reinforced one on either number of trials or number of reinforcements, but not both. If, for example, 50 reinforced responses are to be given and a 50% reinforcement schedule is employed, this group will have 100 trials, and the 100% group will undergo 50 trials. The experimental question revolves around a determination of the relative effects on behavior of non-reinforced trials as contrasted with reinforced ones. One procedure is to equate on trials or responses and allow the partial group fewer reinforcements (see, for example, **12, 28, 49, 59**). If greater resistance to extinction is found for the partial subjects, it occurs despite the weighting of reinforcements in favor of the continuous group. If one is interested in acquisition, the two groups might be matched on number of reinforcements, allowing trials or responses to differ.

Brunswik (**3**) was able to circumvent this difficulty by using the correction method in the T-maze. Thus by reason of a correct response or retracing, food was received on every trial, and trials and reinforcements were equated. In a sense, however, there were no unreinforced trials. Hull and Spence (**27**) have suggested that the correction and non-correction training techniques should have different behavioral consequences. The differences in their data are not striking, and Stanley's (**66**) groups, run with the non-correction method, ranked in the same order as Brunswik's. Absolute differences may, however, exist between the results of the two procedures. In the present context it might be mentioned that Brunswik's rats received all their daily rations in the four or eight trials per day, thus providing a real possibility for gross changes in drive during a given session.

In this connection, it is possible to equate partial and continuous subjects on total *amount* of reinforcement, where trials are matched and number of reinforcements differ, by presenting twice as much food to the partial group on reinforced trials where 50% and 100% schedules are involved. Apparently this technique has not been employed.

Outside the multiple-response situation, where a correction method can be used, the very nature of partial reinforcement negates the possibility of equating both trials (or responses) and reinforcements for continuously and non-continuously reinforced groups, because in order for reinforcement to be partial some trials or responses must go unreinforced.

Another major variable to be considered is number of reinforcements per se.

Resistance to extinction has been used as a major index of the relative behavioral strengths set up by different numbers of partial and continuous reinforcements. Williams (**70**) and Perin (**51**) have demonstrated variation in extinction responses as a function of number of (continuous) reinforcements. Lloyd (**41**) has studied the same variable with periodic and aperiodic reinforcement.

These findings suggest that when a small number of reinforcements (say, less than 50 or so) are given in the bar-pressing situation, an additional variable is introduced in comparing resistance to extinction of partial and continuous groups; obviously, different tasks will have somewhat different asymptotes.

In this connection, Humphreys (**32**) presented one group of rats with 18 reinforcements on 18 bar-depressions and another group with seven reinforcements during 18 responses. Both behavior strengths were presumably increasing rapidly. Furthermore, it hardly seems likely that the consequences of non-reinforced responding in the partial group could have been very strongly conditioned with the small number of reinforcements involved (**59**).

Response strength at the initiation of extinction. The variable of strength of response at the end of learning must be considered in comparing resistance to extinction after 100% and partial reinforcement. If there is any relationship between response strength at the end of acquisition and performance in extinction, the former factor becomes critical in comparing the extinction behavior of partial and 100% groups. If on the Graham-Gagné runway, for example, the partial reinforcement group should exhibit running time several seconds faster than that of the continuously reinforced group at the end of training and then the partial group should yield greater resistance to extinction, this finding could be interpreted as a carry-over effect of behavior strength. It is not the correlation between acquisition and extinction performance that is needed, but rather the r between level of performance during the last few training trials and extinction behavior. Two such values have been reported (**34, 37**) for partial groups only. They consisted of a rho of .54 (S.E. = .27) and a Pearson r of .87(S.E. = .28) between number of non-reinforced responses in a constant period at the end of training and extinction responses. Clearly, more data on this problem are needed.

In the free-responding situation pronounced differential rates have been reported (**36, 37, 62**) at the beginning of extinction for partial as contrasted with continuous groups. At least two investigations (**32, 47**) employing the bar-pressing technique have failed to present any information on this score.

Statistical questions. In addition to the usual statistical problems of design and analysis, partial reinforcement poses one special problem that has received little attention. In several studies where enough data are available (**3, 36, 66**) a moderately high correlation between the mean and the variance seems to be present. Furthermore, there is typically a marked skewness in a given set of responses, particularly in extinction. These situations raise serious doubts as to the applicability of standard statistics for the raw data. Several methods of sidestepping these difficulties are available but have not been widely used in partial reinforcement studies. Non-normal statistics or tests involving no assumptions concerning the distribution can be employed. Another possibility that has been used (**12, 13, 66**) is a transformation of the original measures into common logarithms (for time or response scores) or angles (the arc sin $\sqrt{\text{percentage}}$, for percentages), procedures that tend to normalize the distributions and make the variances less heterogeneous. The interested reader

may consult Mueller's (48) recent article for a comprehensive treatment of these issues, though not in a partial reinforcement context.

A number of variables that need to be considered in order to secure clearcut results in a comparison of partial and continuous reinforcement have been indicated. Failure to control these factors is obviously not an omission peculiar to partial reinforcement studies. Experimental design in this area, however, has not been particularly rigorous. Considerably more experimentation is needed before the effects of these variables can be ignored.

III. Acquisition

In discussing the behavior of partially reinforced animals it is helpful, though perhaps somewhat arbitrary, to consider acquisition and performance separately. "Acquisition" is here viewed as improvement in executing the practiced act until an asymptote is reached, with "performance" beginning at that point. In this section we shall consider experiments wherein the former condition predominated. The first two of these, fragmentary studies conducted by Pavlov (**50**) and Wolfle (**71**), have already been mentioned and therefore need only be indicated again.

Acquisition has been studied most thoroughly in the trial situation. The present section is broken down into parts according to the methodology employed in these instances.

Classical conditioning studies. Cole (6), in the study previously described, was investigating the influence of sophistication (advance knowledge of the procedure) upon conditioning, so some of his subjects were informed verbally concerning certain aspects of the conditioning sequence. Cole's results show clearly that under the conditions of his experiment CRs will be maintained at a surprisingly high level for many trials even though the subject is fully informed of the order of reinforcement and non-reinforcement. His sophisticated subjects were not actually *told* to inhibit the wink on non-reinforced trials, however, so part of the continued responding can perhaps be ascribed to a misunderstanding of the experimental situation, as Cole points out.

The trend of CRs on the non-reinforced trial immediately following the reinforced one is quite alike in both groups, responses occurring less frequently then than on reinforced trials.

One of the earliest partial reinforcement experiments to arouse general interest was Humphreys' (28) 1939 eyelid reaction study. In acquisition, three groups of 22 college students each were trained as follows: (1) 96 trials, 100% reinforcement; (2) 96 trials, 50% semi-randomized reinforcement; and (3) like (2) except that rest intervals were substituted for the non-rewarded trials.

Humphreys equated his groups for reflex sensitivity to light and puff and reported no significant differences between them with respect to number of CRs or magnitude of response during acquisition, although Group II (50% reinforcement) was slightly lower than Groups I and III on both measures. Inspection of Humphreys' data shows that after the first few trials, frequency of CRs in the Group II curve never attains the level of the curve for Group I at any point throughout the entire 96 acquisition trials (grouped in sets of 12 trials). Thus the differences, though small, are highly consistent and suggest somewhat faster acquisition with 100% reinforcement than with 50%. The data seem out of line with Humphreys' conclusion of "presumably chance differences."

In reporting his study of the generalization of the galvanic skin response to various tonal frequencies, Humphreys (**30**) does not present data concerning progressive changes, giving only pre- and post-reinforcement results. "Acquisition" was measured by average millivolts of response following the unreinforced presentation of the four tones, twice each, in a systematically rotated order. Humphreys (**30**) uses the mean of these two values, thereby making it appear graphically that response strength was greater after 50% reinforcement than after 100%. As Fig. 1 in his later article (**31**), based on the same set of data but interpreted in connection with extinction, suggests, this is a misleading procedure, for on the *first* test (extinction) trial there was a slight superiority of the 100% reinforcement group. Thus, the data seem consistent with the position that training does not produce *greater* response strength with partial than with continuous reinforcement.

"Verbal" responses. One of the most provocative partial reinforcement studies is that of Humphreys (**29**) dealing with "expectations." The subject's task was to indicate by marking a record sheet whether or not a second light was to come on following a signal light. Seventy-eight subjects were run through the sequence: 100% reinforcement, extinction, 50% random reinforcement,[3] and extinction. With 100% reward, learning reached a 98% level. With the random series of 50% reward, learning cannot take place. There were, however, several marked and significant deviations from chance in the latter situation. These may be a function of the preliminary training in guessing flips of a coin and/or the restricted randomization of reinforcement.[4] Hum-

[3] But it was not wholly unpredictable, because "The order of 'acquisition' trials for Series II deviated from chance in that no more than two trials of a kind ever occurred together" (**29**, p. 296). This situation could have provided a discrimination for the subjects to learn. A pronounced effect of Humphreys' procedure on his extinction results has been indicated by Detambel (**8**).

[4] Humphreys' "coin flipping" series shows a trend for heads to predominate on odd-numbered trials and tails on even ones (**29**, Fig. 2). This tendency may have carried over into the 50% reinforcement training series, since the same symbols were used throughout the study.

phreys' most striking data are discussed in the extinction section of this paper.

Recently Grant and Hake (17) repeated Humphreys' investigation with certain modifications, including the addition of 75%, 25%, and 0% reinforcement. Their results agree with his in showing that the various groups eventually learned to respond in direct proportion to the frequency of reinforcement.

"Choice" situations. The parallel between the partial-continuous reinforcement dimension and discrimination learning is apparent. Humphreys (**30**), for example, has pointed out that early in discrimination learning reinforcement is practically on a 50% basis, while late in training when the discrimination is well formed it approaches 100%. This is by no means the complete picture, however. Obviously the discrimination learner can improve, i.e., can change the odds of the environment's paying off; in the simple case of partial reinforcement, the organism cannot alter the probability of reward feedback from the environment. In the runway situation of the Graham-Gagné variety, the partially reinforced rat must learn to respond positively to the absence of reinforcement, its concomitants and consequences. It must learn to run on non-reinforced trials as well as on reinforced ones.

Several other instances of the overlap between partial reinforcement and discrimination formation may be cited. These include Cole's (**6**) and Skinner's (**62**) fixed ratio studies, avoidance training, the use of a semi-random partial reward schedule (**29**), and dual- or multiple-response situations where one of the possibilities produces reward less than 100% of the time with the other "choices" paying off somewhere between 0% and 100%.

In the last situation, Mosteller (**44**) has pointed out the possibility of considering the consequences of the various "choices" as different in kind, each kind having a definite probability of occurring. Any kind of consequences may have a distribution of intensity (amount of food or shock) that in turn has a probability distribution varying from small to large. In the more complex instance the interactions of these parameters must be considered.

We are particularly concerned in this sub-section with situations where the experimenter provides the possibilities for responding (e.g. the T-maze) and controls the frequency of occurrence of reward for for each of the "choices." Historically, the first study of this type is Brunswik's (**3**). Large groups of rats were run on a T-Maze with different reinforcement ratios on the two arms. The ratios were: 100%:0%, 50%:0%, 75%:25%, 100%:50%, and 67%:33%.[5] (We have already

[5] The three of these that contain intermediate probabilities on both sides remained unique in partial reinforcement literature until the experiments conducted by Heron (**21**), Calvin (**5**), and Stanley (**66**). Very likely such "in-between" probabilities correspond to the everyday situation more than ratios like 50:0, in which only one alternative is "am-

mentioned certain troublesome areas of this experiment in the methodology section of this paper.)

All patterns except 67:33 resulted in definite learning by the end of the 24 trials. The means and variances are highly correlated, with each of the distributions of errors markedly skewed in a positive direction. In the present context it is noteworthy that the 100:0 rats made fewer errors than the 50:0 animals, although the difference does not quite reach the 5% level of significance.

Instead of extinction Brunswik (**3**) reversed the training of his various groups. Again the 100:0 rats learned most readily; only the 75:25 and 67:33 groups seemed unable to relearn. Humphreys (**32**) interprets these data as extinction, but the operations differ from those usually employed in extinction. Detambel's (**8**) findings, discussed in a later section, suggest that in a different situation the two methods do not yield comparable results.

Brunswik's (**3**) conclusion that response strength in his situation is a compromise between the influence of probability difference and probability ratio does not seem to be a complete statement. The assumption that the 100:0 and 50:0 ratios are identical except for the probability differences is questionable when the "all-or-noneness" of the former is contrasted with the half-time reward on one side of the latter. Similarly, although the ratios 50:0, 75:25, and 100:50 have equal probability differences, there is a confounding of partial reinforcement on the favorable side, on both sides, and on the less favorable side that might be used to explain the progressive increase in errors. The same kind of qualms apply to 100:50 and $67\frac{2}{3}:33\frac{1}{3}$, where the odds in each instance are 2:1. In any event, there is no reason to suppose that any elementary mathematical function of reinforcement percentages will describe the effect.

A detailed analysis of this sort of data has not been performed in terms of the algebraic summation of tendencies strengthened by reinforcement and weakened by its absence, similar to Spence's (**65**) formulation. The additional feature that would have to be included is the absence of 100% reinforcement even for a "correct" response. One mathematical formulation is underway (**44**) on this problem.

Stanley (~~**66**~~) ran five squads of seven matched rats on a T-unit with these ratios: 100:0, 50:0, 75:25, 100:100, and 50:50. A criterion of learning was employed such that 13 days of eight trials each were involved on the average. These were followed by extinction trials. The non-correction method was used. His design permitted analysis of the total variance or covariance into three parts, that attributable to blocks, reinforcement ratios, and error. Number of reinforcements

biguous." The analogy of these ratios to certain of Lewin's (40) types of conflict is apparent, e.g., 100%:50% and approach-approach.

were matched within each *block*, being based on the number received by the 100:0 rat in that block. Thus, number of trials varied systematically from group to group.

In terms of errors, the ranking of the three groups was similar to Brunswik's (**3**): 75:25 most, 50:0 next, and 100:0 least. For these three groups speed of learning was positively related to number of reinforcements not merely trials, as Humphreys (**32**) has contended. With reinforcements equated, the 100:0 and 50:0 groups did not differ significantly in regard to wrong turns *per trial*, but the 75:25 animals were distinctly more erratic than either.

Denny (**7**) ran 100:0 and 50:0 groups on a T-maze to test his hypothesis that the almost equal learning level reported by Humphreys (~~**32**~~) for partially and continuously reinforced groups when number of trials was equated but reinforcements favored the 100% group could be attributed to secondary reinforcement from the goal box on the nonreinforced trials.

Denny interprets his acquisition findings as supporting the secondary reinforcement hypothesis. Using the initial trial of each day, he shows that when secondary reinforcement is allowed to operate (control condition) acquisition proceeds almost as well under 50% as under 100% reinforcement. Also, the expected (and significant) difference in favor of continuous reinforcement is found when secondary reinforcement is minimized. If the first *two* daily trials are used for the control groups, however, the superiority of the 100% rats reaches the 10% level of significance. This finding suggests that the 100% control group may have learned more rapidly than the 50% control group.

The interaction of partial and secondary reinforcement has been the topic of several other investigations of the choice variety. Ehrenfreund (**10**) has shown that the secondary reinforcing properties of goal boxes are significantly weaker when previously associated with partial (50%) reward than after the same number of continuously rewarded trials. The number of primary reinforcements was 40 for the 100% group and 20 for the 50% rats.

In a similar study, Saltzman (**55**) found superior learning for his partially reinforced rats when working for the secondary reward cues. The 100% group received 25 rewarded runs; the partial animals had the same training plus 14 interpolated non-reinforced runs. In a second study interspersed trials on the original runway produced equal or superior performance for the 100% group.

The number of reinforcements employed in both these studies is noteworthy, since a similar investigation (**19**) has shown an increased tendency to learn to run to a secondarily rewarding goal box as the number of continuous, primary reinforcements in training increased from 25 to 50 to 75. Whether or not this parameter acts differentially in the partial-continuous context is an experimental question.

In this connection a recent study (**14**) showed that rats will press a bar more frequently for a secondary reinforcing stimulus when the acquired reward property stems from previous pairing of the stimulus with aperiodic as contrasted with continuous primary reinforcement. The mean number of bar-pressing responses in 45 minutes was 39 for the 100% group and 57 for the aperiodic group. The difference just fails to reach the 5% level of significance; the number of cases was small, four in the 100% group and five in the aperiodic squad.

Employing various combinations of 10 electric light bulbs on a vertical board, Calvin (**5**) studied the written predictions of college students as to the occurrence or non-occurrence of a buzzer following each combination. One of the combinations was succeeded by the buzzer a greater or lesser proportion of trials.

Calvin concluded that learning in this situation depends on the probability of reinforcement for the particular pattern and also on the probabilities characteristic of the other stimulus compounds.

Only Heron (**21**) has conducted an investigation in which rats reinforced periodically with respect to time were presented with alternatives set up by the experimenter. After preliminary training of 48 rats to press a single lever in order to receive a pellet of food and then to operate two levers, he set the right lever to "pay off" a pellet every three minutes and the left one each six minutes. Under these conditions the frequency of responding was about the same on the two levers until the seventh hour, when it increased on the right lever. Thereafter, frequency was maintained at a relatively high level on the right lever and decreased slightly and gradually on the left one to the end of the 17 one-hour periods. The ratio of responses on the two levers was about what is to be expected from Skinner's (**62**) essentially linear (negative) relationship between frequency of responses and size of time interval between reinforcements under the periodic regimen.

Straight runway. The two experiments that Finger (**12, 13**) reported in 1942 have been challenged by Lawrence and Miller (**39**) on the grounds that interchanging the goal and starting boxes, with the rat still inside, eliminated distinctive cues for the incompatible responses of searching for food and running.

With regard to acquisition under a partial reinforcement schedule, Finger's results are fairly clear. His partial reinforcement groups were all reliably slower than the 100% animals. Finger's data on acquisition are generally in line with other results in showing somewhat more rapid learning with continuous reinforcement than with partial. It does not seem that the lack of distinctive cues was acting differentially, although its possible confounding effects cannot be partialed out. By analogy to the Lawrence-Miller argument, the learning of the partially reinforced rats may have been somewhat retarded on the trials following non-reinforced runs since there would be conflict between the tendencies

TABLE II

A Comparison of Acquisition Under Continuous and Partial Reinforcement

The heading "Direction of Difference" means that the group listed under "1" learned more readily than the group given under "2." A blank space under "P-Level" indicates that the probability of the difference did not reach the 5% level of confidence. An asterisk at any point in the body of the table refers to data estimated or calculated by the present writers.

Investigator and Situation	Direction of Difference 1	2	Ratio of Smaller to Larger	P Level	Comment
Brunswik (3)	100:0	50:0	.45		
T-maze	100:0	75:25	.35	5%	
	100:0	100:50	.25	1%	
	100:0	67:33	.15	1%	
Denny (7)	100:0	50:0	.96		Control
T-maze	100:0	50:0	.69	1%	Experimental
Finger (12) Runway	100	50	.65*	1%	
Finger (13) Runway	100	50	.82	1%*	Common logs
Grant & Hake (17) Verbal Responses	100	25, 50, 75	—	1%	Abstract
Humphreys (28) Eyelid	100	50	.97		Frequency and magnitude
F. Sheffield & Temmer (58) Escape-avoidance	Escape	Avoid.	.46	1%	
V. Sheffield (59) Runway	100	50	.99*		
Stanley (66)	100:0	50:0	.59	1%	Errors
T-maze	100:0	75:25	.43	1%	Errors
	100:0	50:0	.54	1%	Trials to
	100:0	75:25	.57	1%	criterion

to look for food and to run. There is a slight indication of this influence in Finger's data. The effect would be diluted, of course, by learning.

Though presumably she controlled "secondary reinforcement" somewhat by removing the food cup on non-rewarded trials in her runway situation, Sheffield (**59**) found only a very slight acquisition difference favoring the 100% group over the 50% subjects ($P = .30$). This finding held for both massed and distributed practice, with inter-trial intervals approximately 15 seconds and 15 minutes, respectively.

Escape and avoidance training. In a recent investigation that has broad theoretical implications, Sheffield and Temmer (**58**) found that rats learned more readily to escape from a shock situation than to avoid it. The learning curves rose together, after which the curve for avoidance training dropped gradually to a lower level. Since avoidance means a lessening of reinforcement, while escape involves 100% reinforcement, these results are in line with other acquisition data discussed in this section.

Tabular Comparisons. Table II contains comparative data from the relevant experiments referred to above. In not a single instance did subjects learn better under partial reinforcement than under continuous. Only Denny (**7**), Humphreys (**28**), and Sheffield (**59**) failed to find significant superiority for 100% reinforcement in *any* phase of their studies.

Over-all, it can be said with considerable assurance that *acquisition proceeds somewhat more rapidly and reaches a higher final training level under continuous reinforcement than under partial reinforcement.* With prolonged training partially reinforced groups may approach the same level of proficiency attained earlier in training by continuously reinforced subjects.

IV. Performance, Maintenance and Retention

In this paper a distinction that is somewhat arbitrary has been drawn between acquisition and performance. By acquisition we have meant the progressive changes in behavior prior to the stabilization of responding at a more or less constant level. In the bar-pressing apparatus, for example, it is frequently difficult to determine when acquisition occurs. One response of the specified variety (magnitude, direction, etc.) may be a sufficient (as well as a necessary) condition for starting the behavior off at a relatively constant and stable level. Certainly for practical purposes one can distinguish learning from already established behavior. In this section we are concerned with the role played by partial reinforcement in maintaining behavior previously acquired.

One of the classical studies in this area has been reported by Skinner (**62**, p. 127 ff.). After previous conditioning and participation in other

experimental activities, four rats were run in a rotated order under several different periodic reinforcement regimes: three, five, seven, and nine minutes. Each rat underwent two one-hour periods on successive days at each interval. The mean number of responses per hour were, for the four periodic intervals respectively: 319, 266, 220 and 169. The relationship is essentially linear over the range of four points involved.

A second kind of partial schedule, called fixed ratio reinforcement, was also developed and studied by Skinner (62). The operations defining it consist of presenting a reward for a fixed number of responses. Thus a rat might be reinforced after every 20th bar-depression. Skinner's basic experiment (62, p. 286 ff.) consisted of running six previously conditioned rats through a random order of fixed ratios: 48, 96, or 192 responses for one reinforcement. The characteristics of the curves are striking: (1) Just after receiving food for a response there is a pause that increases as the ratio decreases. (2) There follows a period of gradually increasing responding that (3) seems to reach a stable state shortly before the next reinforcement. (4) The over-all slopes of the cumulative response curves appear to steepen as the number of responses per reinforcement increases, but the effect is not pronounced. This finding may be related to the fact that a greater portion of the time is devoted to eating at the larger ratios. In an hour almost three times as many pellets are received in the 48:1 case as contrasted with the 192:1 regime.

Eating time may be somewhat compensated for by the longer pause after eating at the lower ratios, but the relationship between ratio and rate is by no means a simple one such as that holding between rate of responding and length of time interval under a periodic reinforcement schedule.

It should be noted that the behavioral effects of periodic and fixed ratio reinforcement are opposed. In the first procedure, increasing the time interval between reinforcements produces a decreased rate of responding. Under fixed ratio reinforcement, increasing the number of responses per reinforcement tends to yield a higher rate overall. In addition, the qualitative characteristics of the curves are markedly different. Skinner's cumulative response curves under periodic reinforcement are smooth, lacking the discrete, step-wise nature of the fixed ratio curves. Fixed ratio reinforcement strengthens rapid responding; periodic reward ultimately strengthens responding.

Chronologically, the next systematic investigation of performance involving partial reinforcement was Brodgen's (1). Three groups of four dogs were employed. One group had leg flexion conditioned to bell with the US of shock. The second group had similar initial training to a high level of performance followed by the substitution of food for shock. The third group underwent a classical conditioning routine of salivation to the CS of bell and the US of food. After conditioning had attained a 100% frequency for all three groups, with continuous rein-

forcement, partial reinforcement was instituted. This consisted of 100 trials under each of the following frequencies: 80%, 60%, 40%, and 20%, with the preceding schedule of 100% available as the continuous-reinforcement baseline.

In all three groups frequency of CRs with 20% reinforcement was less than that of the 100% condition—significantly so in all but the shock-flexion group. The latter animals showed a progressive *increase* in frequency of CRs as reinforcement decreased from 100% to 40% followed by a drop in CRs at 20%. The other two groups yielded a more or less progressive decline in frequency as reinforcement decreased. The lowest mean frequency of response attained was above 80% for all three groups. Latency and magnitude of conditioned salivation yielded generally decreasing trends as frequency of reinforcement decreased from 100% to 20%.

We have commented in Section II on the possibility that the progressive error inherent in Brogden's design and the degree of overlearning employed operated to maintain the response at high frequency throughout and thereby reduced any decremental effects of the lower frequencies of reinforcement.

The poorer conditioning with unavoidable shock (100% reinforcement) in Brogden's study is consistent with other findings (**24**, **57**). As Sheffield (**57**) has suggested, one possible action of unavoidable shock is to condition to the experimental situation other behaviors (or "states" producing them) that are incompatible with the response studied. Brogden himself reports decreased "vocal response, diffuse behavior, and general restlessness" (**1**, p. 422 f.) with decreased frequency of (shock) reinforcement.

The conclusion from Brogden's data is clear: Low frequency of partial reinforcement produces a response decrement.

Murphy (**49**) concluded that there was little relationship between rate of responding and frequency of reinforcement per response in his pinball situation. The mean number of responses per minute for continuous reinforcement, 1/5, 1/10, and 1/20 were: 17.4, 15.2, 15.6, and 16.4. (These data have been averaged by the present writers from Murphy's several conditions.) In 70% of the measurements, 100% reinforcement yielded rates above those for the fixed ratios. It seems that human subjects will continue to work almost as hard for infrequent "reinforcements" as for frequent ones of the kind Murphy used. The specific action of the variables of sequence and number of reinforcements, previously noted, cannot be partialed out. It is surprising that the subjects did not verbalize the situation and speed up their responses if they were really "working for" the light. It is noteworthy in this connection that the responses per minute when the light-reinforcement was given either continuously or intermittently is slightly *lower* than in preliminary practice periods at the beginning of each session when the

subjects recieved no light and were instructed to operate the plunger at a "natural or comfortable" pace and force. Apparently "reinforcement" of the light added little or nothing to the instructions as far as number of responses per minute was concerned.

Where the organism is free to respond, as in situations involving bar-pressing or a similar instrumental act, it is difficult to isolate an acquisition phase. Thus these studies bear mostly on performance during conditioning. Two investigations, one involving periodic reinforcement (**37**) and the other aperiodic (**36**), may be mentioned. In the first instance the drive involved was thirst and the response was bar-pressing by rats. Performance, measured in terms of rate of responding, was significantly higher under continuous reinforcement than under either a one-minute or a two-minute periodic schedule. This finding bears particularly on the question of response strength at the beginning of extinction. One might question whether differences in resistance to extinction—favoring groups partially reinforced in conditioning—are attributable merely to a carry-over of greater response strength. In these data, the situation was clearly and significantly reversed, so extinction differences favoring intermittent reinforcement cannot be accounted for by differential rates of response at the end of conditioning.

The second study (**36**), involving aperiodic reinforcement, raises a related problem. The pecking response of pigeons was employed with hunger as the drive. The aperiodic schedule averaged one reinforcement every minute. Rate of responding was approximately five times as high for the aperiodically reinforced birds as for the continuously reinforced ones in a constant period of time just prior to extinction. This comparison is not a fair one, however, since about one-half of the time was devoted to eating in the case of the continuous schedule and only about 10% for the aperiodic one. Even when the groups are matched on this score, though, the rate of responding was more than twice as high for the partial reinforcement subjects. Differences in extinction were in the same direction and of even greater magnitude.

In any event, the bar-pressing study cited previously (**37**) makes it clear that response strength under performance conditions is not the only variable operating to produce differential extinction effects after partial and continuous reinforcement.

In the present performance context might be mentioned Humphreys' (**30**) finding that generalization of GSR to tones as far removed as 26 *j.n.d.*'s from the reinforced tone takes place much more readily following partial reinforcement than after continuous reinforcement.

Mosteller and Nogee (**45**) have recently studied gambling behavior in a situation similar to partial secondary reinforcement. The subject is shown a poker hand and is told what the (statistically) "fair" odds are. He either bets or does not bet a nickel against certain odds of drawing a hand exceeding the one shown to him. Latency of response

is one measurement made. If a bet is taken the hand is dealt and money is paid or received. By varying the betting odds on a particular hand the point is found at which a given subject takes the bet 50% of the time. By using a number of different hands, a series of such indifference points are found. These are used along with a theoretical equation to obtain a utility curve for money. The data were still under analysis at the time of writing, but the trends suggested that highly motivated (poorer economically) subjects were rather consistent about taking unfair bets, while less motivated subjects consistently wanted better than fair odds.

Skinner (**63**) has recently found that the pecking response of pigeons is maintained at a relatively constant rate during the major portion of an eight-hour performance session. The aperiodic schedule averaged a reinforcement once every five minutes, and during the period around 50,000 responses were given. There appeared to be only a moderate decline in frequency of responding toward the end of the session.

One systematic study of long-term retention might be mentioned. Skinner (**63**) has tested small squads of pigeons on a partial reinforcement schedule after intervals of two and four years of no experimental activity. There appears to be little if any loss or change in the response.

Partial reinforcement procedures—particularly periodic and aperiodic—have served as valuable baselines in the study of the effects of several variables on behavior. The stability of responding under these regimes provides a sensitive measure of the action of such factors. An additional advantage of these partial reinforcement schedules is the opportunity for relatively prolonged single experimental sessions by way of staving off satiation effects. Some of the variables that have been studied under these conditions include drugs such as benzedrine, phenobarbital, insulin, and caffeine (**62, 64, 69**); amount of reinforcement (**35, 62**); drive (**22, 34, 62**); and quantitative variations in the response (**62**).

Empirical principle. The data seem to be in condition to derive an empirical principle that is, of course, open to further experimental test. A number of variables are operative both in experimental and every-day situations that will cut across the present generalization. The amount of reinforcement may, for example, be critical. It seems questionable that without an exceptionally high drive human beings would work as hard for exceedingly small amounts of money administered after every response as they would for large amounts given occasionally. In any event, the data concerning maintenance of behavior under partial reinforcement are fairly clearcut. The following characteristic of performance under a partial reinforcement schedule emerges: *All other things equal, performance under a partial reinforcement schedule tends to be somewhat lower than that under a continuous reinforcement one*

as measured in terms of single responses. The behavior is stable under a partial reinforcement schedule. Instances can be found indicating either higher or lower response strength in the partial instance, but, at least for small frequencies of reinforcement, the typical response strength is lower with partial reinforcement than with continuous reinforcement.

One exception to this principle may be indicated, namely, fixed ratio reinforcement in the free-responding situation. Under this schedule there is some increase in rate of responding as frequency of reinforcement is decreased, i.e., as number of responses per reinforcement is increased. As Skinner (**62**, pp. 274-300) has pointed out, this procedure involves a special discrimination in which rapid responding is strengthened since it brings reward sooner. The effect is not as pronounced as the decrease in rate found with increased time interval in periodic reinforcement. Both the fixed ratio and random ratio reinforcement schedules need more investigation. This question is discussed further in Section VI.

The question of practical vs. statistical significance arises in connection with the lower response strength under a partial reinforcement regime. In everyday behavior it does not seem of great consequence whether partial reinforcement produces a stable response level below that of continuous reinforcement at a statistically acceptable level (as is indicated by several studies). The important point is usually for occasional responses to occur. Partial reinforcement unquestionably accomplishes this.

V. Extinction

Extinction has been a ubiquitous measure in the comparison of partial and continuous reinforcement. A large number of studies have employed this criterion of response strength in comparing the two conditions of reward and in evaluating the influence of partial reinforcement on behavior. The maintenance of behavior in the absence of reward after a partial reinforcement training regime raises several theoretical problems that are treated in a later section. The practical implications are far-reaching. It can be said that extinction has been the main mirror reflecting response strength in the partial reinforcement situation.

The data regarding the comparative effects of partial and continuous reinforcement on extinction are summarized in Table III. It includes All the studies the authors could search out that involved the partial-continuous comparison. Like any such summary it is incomplete and needs elaboration, but the trend of the findings from a number of different experimental situations is clearly represented.

TABLE III

A Comparison of Resistance to Extinction After Continuous and Partial Reinforcement

The heading "Direction of Difference" means that the group listed under "1" yielded greater resistance to extinction than the group given under "2." A blank space under the heading "*P*-Level" means that the difference did not reach the 5% level of confidence. A dash indicates that the data were not given in the original report. An asterisk refers to data calculated or estimated by the present writers. Consistent data for more than one comparison have been combined.

Investigator and Situation	Direction of Difference		Ratio of Smaller to Larger	P Level	Comment
	1	2			
Denny (7) T-maze	100:0	50:0	.96		
Finger (12) Runway	50	100	.56	2%	
Finger (13) Runway	100	50	.97		
Gilinsky & Stewart (15) Learning Mazes	50	100	—	—	Abstract
Grant & Hake (17) Verbal Responses	25, 50, 75	100	—	1%	Abstract
Grosslight & Child (18) Plunger-pull	Partial	100	.49, .10	(6%), 1%	No. of S's, trials
Humphreys (28) Eyelid	50	100	.47	1%–5%*	Frequency & magnitude
Humphreys (29) Verbal Responses	50	100	.55*	1%*	
Humphreys (31) GSR	50	100	.68*	1%	
Humphreys (32) Bar-pressing	50	100	.71	1%*	Equal no. of reinforcements
Jenkins *et al.* (36) Key-pecking	Aperiodic	100	.26	1%	
Jenkins & Rigby (37) Bar-pressing	Periodic	100	.48	2%–4%	Equal no. of reinforcements
Mowrer & Jones (47) Bar-pressing	25, 33, 40, 50	100	.60	1%–2%	
Murphy (49) Pinball	5, 10, 20	100	.31*	1%*	
F. Sheffield & Temmer (58) Escape-avoidance	Avoid.	Escape	.47	1%	
V. Sheffield (59) Runway	50 100	100 50	.76 .95	1%–3%	Massed Spaced
Stanley (66) T-maze	Partial Partial	100 100	.36 .57	1% 1%–3%	Trials Errors

~~The consistency of the data in this table is striking.~~ Of the 17 separate studies listed, only three failed entirely or in part to find extinction differences significant at the 5% level of confidence favoring the groups partially reinforced in training over the continuously rewarded ones. ~~About 35 comparisons are condensed in Table III.~~ Only four of the separate measures yielded differences in a direction opposed to this trend, and in none of the ~~three~~ two experiments concerned did the differences reach the 10% level of confidence. ~~All three~~ Both have special aspects and are discussed in the following paragraphs.

The first exception to the noted trend is in Denny's (**7**) study, where slight differences favoring the continuous groups were found in resistance to extinction for experimental subjects for which secondary reinforcement had been minimized. In the control groups the results were similar, but again not significant.

The action of an important variable in Denny's study has been demonstrated by Sheffield (**59**). She found that resistance to extinction was slightly (but not significantly) better after continuous reinforcement when the training trials had been *spaced*. With *massed* practice the findings were reversed and the typical difference favoring the partial group was statistically acceptable. The spacing variable certainly affected Denny's results, since he interpolated intervals from 20 to 30 minutes between training trials where Sheffields' spaced condition was only 15 minutes. ~~The theory behind the Sheffield experiment is discussed in detail in Section VII.~~

The ~~fourth~~ 3rd exception to the trend in Table III (again not attaining statistical significance) is found in the second of two studies by Finger (**13**). The continuously reinforced groups exceeded the partial subjects in trials to reach a criterion of non-response by a very small (and insignificant) margin. The action of lack of distinctive cues inherent in Finger's procedure of employing the goal and starting boxes interchangeably has already been discussed. The superiority of the continuous animals in Finger's second study is of special note since the findings were the reverse of those in his first investigation. The major difference between the two experiments seems to be the interpolation of a 24-hour interval between acquisition and extinction in the second case. Since the difference in the second study was minute and unreliable, this finding could be attributed to sampling. In the first study the difference in the opposite direction was significant at better than the 2% level of confidence. It is noteworthy that the partial animals were slightly faster starters on the average, but slower runners in the extinction of the second study.

In any event, Finger's data cannot be considered a serious contradiction of the overall principle, since the interaction effects of partial and derived reinforcement are not determinable without further investigation.

One other study not included in Table III requires comment, namely, the investigation by Keller (**38**) of the effects of the sequence of partial and continuous reinforcement. The methodological import of these data has already been pointed out. The post-continuous group significantly exceeded the post-periodic group in responses in the first five minutes of extinction; the responses of the partial animals exceeded those of the continuous ones in the second and third hours. After three hours the post-periodic group showed a very slight superiority (1%). These findings are consistent with those from other studies (**37**, **62**) showing more rapid responding early in post-continuous extinction than in post-periodic extinction. The fact stands out, however, that when 30 continuously reinforced responses were followed by 30 periodic ones or vice versa, the asymptote in three hours of extinction was inappreciably different in the two cases. Recent additional evidence (**9**) on the continuous-partial sequence supports Keller's findings, except that the rats starting extinction after continuous reinforcement slightly exceeded the other group in total extinction responses (ratio of 1.2). A special aspect of this investigation was that initial conditioning with 20 continuous reinforcements and one hour of extinction preceded the initiation of the experiment proper.

In both of these sequential studies the question of the effects of larger numbers of reinforcements is still open to experimentation.

There remain several other studies and points that need treatment. The earliest extinction comparison of periodic and continuous reinforcement has been reported by Skinner (**62**). Although not providing numerical information, he notes that the post-periodic cumulative extinction curve in the bar-pressing situation as compared with the post-continuous one rises more slowly, reaches a higher asymptote (with prolonged extinction), falls off more slowly, and lacks the cyclic fluctuations which are attributed to emotional effects in the continuous case. He concludes that "no amount of continuous reconditioning will yield an extinction curve of the height obtained through even small amounts of periodic reconditioning" (p. 138).

Further support for these statements may be found in a recent investigation (**37**). It was shown that 90 reinforcements administered according to a two-minute periodic schedule yielded an appreciably greater number of extinction responses than did 2400 continuously reinforced responses (ratio of 1.4). The continuously reinforced subjects had the advantage in both number of reinforcements and total number of conditioned responses as shown by ratios of about 27 to 1 and 5 to 1, respectively.

As regards extinction after fixed-ratio reinforcement, Skinner's curves show exceedingly high rates of responding, from which he concludes that "if a response is going to be emitted at all, it will come out as soon as possible" (**62**, p. 295). The rate curves in extinction are characterized by steep slopes and abrupt cessation of responding. Data

comparing extinction after a wide range of fixed ratio reinforcements are not available, but the findings of Mowrer and Jones (**47**), who used ratios from 1/1 up to 1/4, are contained in Table III. Even with this narrow range they found large differences in single extinction responses favoring the smaller fractions.[6]

Several studies condensed in Table III do not provide information on differential responses strengths at the initiation of extinction. Mowrer and Jones (**47**), for example, found that resistance to extinction varied inversely with the size of the fixed ratio reinforcement, but they did not report response strength at the end of conditioning. From other data (**62**) it would appear that higher fixed ratios are characterized by higher rates of responding.

One special point may be made here. Resistance to extinction is usually *greater* after partial reinforcement, and typically, response strength in conditioning is *less* for the same case. For example, periodic reinforcement yields a lower rate of responding in conditioning than does continuous reinforcement (at least when the periodic interval is fairly long); this lower rate appears to carry over into extinction. When the latter is prolonged, the periodically reinforced subjects ultimately exceed the continuous ones in total number of responses, although rate of response early in extinction may be greater for the continuous subjects (**37**, **38**, **62**). With a truncated extinction, however, the situation may be reversed. When the higher response strength characterizes the partial reinforcement instance in conditioning (as in fixed ratio situations), the potential effect of this variable cannot be ignored. From a practical standpoint, as has been pointed out, the relative response strengths in performance may be of little consequence as long as the desired behavior occurs occasionally.

Further questions are raised by the rather small number of reinforcements administered by several investigators (**7**, **12**, **13**, **32**), a point previously treated. It will be noted that the comparison in Table III for Humphreys' (**32**) bar-pressing study is based on groups with equal numbers of reinforced responses. It will be recalled that two groups received 18 reinforcements, in the one case in 52 responses and in the other in 18. The other two groups were reinforced seven times in either eighteen or seven responses. In a comparison of the two groups matched on total number of trials (18 responses), the continuously reinforced

[6] The abscissa in Figure 3 of the Mowrer-Jones (**47**) article should be percentage of total number of bar-pressings reinforced during final training and the ordinate, correct as shown, the total number of bar-pressings during extinction. The abscissa actually consists of the five reinforcement ratios used in the experiment: 1/1 (ratio of one reward for one lever-pressing, or 100% reinforcement); 1/2 or 50%; 1/2.5 or 40% (randomly administered); 1/3 or 33 1/3%; and 1/4 or 25%. The authors have plotted the *reciprocals* of these ratios. Replotting the graph with the percentages given above fails to produce even an approximation to the linearity claimed by the investigators.

rats were slightly superior in extinction behavior (by an average margin of less than one response), with the group averages around 25. But they also had two and one-half times as many reinforcements. Increasing resistance to extinction with increasing numbers of reinforced responses in conditioning is a point previously discussed in detail. In this case there is clearly a confounding of the contribution of number and pattern of reinforcements. For this reason, these data were omitted from the table. The unknown but possibly differential effects of small numbers of reinforced responses across the partial-continuous variable have been suggested previously.

Detambel (**8**) has put forward a cogent criticism of Humphreys' (**29**) study in which subjects were to guess whether a second light was to come on following a signal light. The problem is posed by the fact that Humphreys' acquisition curves begin near 50%, as would be expected from the instructions, including preliminary exposure to guessing flips of a coin, but in extinction the curves for both conditions of reinforcement (50% and 100%) drop in a few trials to a near-zero level. According to Detambel's analysis, Humphreys' instructions set the stage so that whenever the subject's "yes" was followed by the second light the "yes"-response was strengthened. Whenever the second light failed to occur following a "no"-response, this alternative behavior was reinforced. Thus in extinction the complete absence of the second light strengthened the "no"-response to such an extent that it reached a level near 100% frequency, while the competing response of "yes" was never reinforced and approached zero.

Detambel tested his hypothesis in a situation consisting of a pair of keys and one light. A correct response was a key closure that turned on the light. The subjects were instructed to press the key they thought would turn on the light. Two groups parallel to Humphreys' were run (100% and 50% reinforcement, with Key A correct in training and Key B correct in extinction) along with a 50% and a 100% reinforcement group exposed to no reinforcement in extinction. A minimum of 120 extinction trials was given, with 25 subjects in each of the four groups.

When the procedure resembled Humphreys', the extinction curves were similar to his. Striking differences were found when reinforcement of the incompatible response was omitted in extinction; both curves dropped towards a chance level (50%), with a faster approach following 100% reinforcement. These findings clearly support Detambel's contention that Humphreys' procedure was contaminated by the introduction of an incompatible response so that extinction consisted of the differential weakening of one response and the strengthening of the other.[7]

[7] We have not intended to imply support for either an interference or an adaptation theory of extinction in this and related contexts. Extinction may well involve the learn-

We have already indicated the difficulty of evaluating the effect on Humphreys' results of running the same group of subjects through the sequence: conditioning with 100% reinforcement, extinction, reconditioning with 50% reinforcement, and re-extinction.

Using independent groups, Grant and Hake (17) have enlarged on Humphreys' findings by employing 100%, 75%, 50%, 25%, and 0% reinforcement. Their data show greater resistance to extinction after the partial schedules.

There still remain several studies dealing with the consequences of partial reinforcement for resistance to extinction. These have not been included in Table III either because they did not involve a continuous-reinforcement baseline or because they employed special reinforcing stimuli.

The first of these is a systematic investigation by Estes (11) of the effects of shock on behavior in the bar-pressing situation. Estes' administration of periodic shock was a special case of the procedure as defined by Skinner (62), since the former's involved presentation of shock for all responses within a portion of the total exposure time. Thus in one case (Experiment K) for one minute of every four all bar-depressions were followed by shock, while during the remaining three minutes responses were neither rewarded nor punished. Straight extinction followed this treatment. Control groups received shock for every response or extinction in a similar period. Two separate studies yielded comparable data. The initial effect of periodic shock was less than that of shock for every response, i.e., more responses were given under the periodic shock condition. In extinction following these treatments, however, the number of responses was consistently smaller for the periodically shocked groups than for those given shock for every response. About 50% more responses were given in extinction after continuous shock in one experiment and 15% in the other.

In his two-bar situation previously described, Heron (21) gave 12 days of extinction to one-half of the rats on the right lever and to the other half on the left lever with the other one yielding reinforcement once every three minutes. Responses in extinction were more frequent on the formerly three-minute reinforcement lever for the first four days.

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ing of incompatible responses or of not-responding. The subject is certainly doing something else when it fails to respond in extinction. Rather, we have intended to stay close to the common usage implied by the term extinction: omission of the reinforcing stimulus and the decreasing response strength that accompanies this operation. Our interpretation is that Humphreys (29) did not follow this model, since his extinction procedures involved the strengthening of an incompatible response that (if it existed at any strength at the initiation of training) was presumably weakened during the conditioning of the alternative behavior. Brunswik's (3) relearning situation was omitted on similar grounds. Detambel's (8) experimental analysis indicated that in Humphreys' case the two procedures lead to very different consequences.

On the fifth and sixth days the average number of responses on the two levers was about the same. On days 7–12, more responses were given on the lever formerly producing reinforcement once every six minutes. These findings are consistent with other data (**37, 38, 62**) in showing that the low rate for the low frequency of reinforcement in conditioning is maintained in post-periodic extinction so that if the latter is prolonged the ultimate number of responses exceeds that for the group (or lever) more frequently rewarded in training.

It should be noted that Heron's study does not involve a simple case of extinction; responding on the non-reinforcing lever was presumably maintained to some extent by three-minute periodic reinforcement on the other lever. Extinction effects generalized in the opposite direction: The number of responses on the lever paying off every three minutes dropped. The former effect may account for the relatively high rate of responding on the "extinction" lever even after 12 extinction periods. Also, the fact that for half the subjects extinction involved the additional shift from six- to three-minute periodic reinforcement on the left lever must be considered.

Another study relating partial reinforcement to extinction was performed by Cole (**6**). Fixed-ratio reinforcement was employed in conditioning either every other trial or every fourth one. The frequency of CRs in extinction after 50% reinforcement followed a roughly stepwise function although some CRs appeared on all early trials. In this connection, it should be noted that the discrimination of blinking on alternate trials was not particularly well established when extinction was instituted even though some of the subjects were explicitly instructed regarding the nature of the treatment.

Eight subjects were reinforced on the 1st, 5th, 9th, etc. trial. In extinction the effect of this training was very pronounced. Except for one set of points (in 28 trials) fairly late in extinction, pronounced peaks in frequency of CRs appeared exactly every fourth trial starting with the first extinction trial. Frequency of CRs decreased to a minimum by the third or fourth extinction trial of a given block and rose greatly on the first trial of the next block. The orderliness of the data is striking. Overall, of course, the general trend was towards decreasing frequency of CRs. It should be noted that the eight subjects were fully acquainted with the procedure except that they were not instructed as to the beginning of the extinction series. It is difficult to evaluate the effects of the earlier 100% reinforcement and extinction on the later training.

A comparison of periodic and aperiodic reinforcement in resistance to extinction has recently been performed by Lloyd (**41**). The total number of responses for groups given larger numbers of reinforcements was about the same in four one-hour periods of extinction, although the post-periodic rats produced significantly more bar-pressing responses (at the 5% level) in the first extinction hour.

Finally, an empirical generalization following from the material of this section can be stated. It is quite straightforward in view of the consistency of the data: *All other things equal, resistance to extinction after partial reinforcement is greater than that after continuous reinforcement when behavior strength is measured in terms of single responses.* A necessary condition for the operation of this principle appears to be massing of training trials or responses, at least with infra-human subjects.

The reasons for qualifications concerning measures in terms of single responses and massing of training trials are set forth in the following section on theory. The explanation of the greater resistance to extinction of behavior based on a partial as compared with a continuous reinforcement schedule is also deferred to the theory section.

The practical question of how best to build a response to withstand elimination in the absence of primary reinforcement is clearly answered by these data: *Administer the reinforcements according to a partial regime.*

VI. Theory

Explanations of partial reinforcement phenomena fall into two mutually exclusive categories: (1) those based on aspects of stimulus-response learning theory and deductions therefrom; and (2) positions referring to cognitive processes and the acquisition of expectations and hypotheses. This breakdown will be followed in the present section.

The facts deriving from a comparison of partial and continuous reinforcement that a complete theoretical treatment must account for are these: (1) more rapid acquisition with 100% reinforcement, (2) a somewhat higher level of performance with 100% reinforcement, and (3) considerably greater resistance to extinction after partial reinforcement.

S-R learning theory. A straight S-R interpretation, that reward strengthens a response and omission of reward weakens it, would predict a weaker response with partial reinforcement than with 100% reinforcement in acquisition, performance, and extinction. The facts of post-partial extinction are clearly opposed to this position. By the addition of one further stimulus-response learning-theory concept, it appears that all the facts of behavior under a partial reinforcement schedule can be accounted for. This principle is the common one of generalization, that the more similar the stimulus pattern to the one conditioned, the greater the response strength. These matters are discussed in the following paragraphs.

The first systematic treatment of partial reinforcement appears to have been Skinner's (62). In discussing fixed ratio reinforcement, Skin-

ner pointed out that where reinforcement occurs after a pair of responses, the pair should be treated as a unit. He says: "As a rather general statement it may be said that when a reinforcement depends upon the completion of a number of similar acts, the whole group tends to acquire the status of a single response, and the contribution to the reserve tends to be in terms of groups" (p. 300). That is, if two bar-depressions are required to produce reinforcement, and a single reinforcement sets up 10 responses, then 20 single responses (10 pairs) can be expected in extinction after one reward.

According to Skinner, the non-reinforced responses provide a stimulus situation which reinforcement ultimately follows. In other words, in the fixed ratio instance they come to act as stimuli setting the stage for the occurrence of reinforcement. This property is acquired in the same way that external stimuli in a discrimination come to set the occasion for the appearance of reward. The early (non-reinforced) responses in a series such as in the fixed ratio case produce cumulative discriminative stimulation in the presence of which a response is followed by reinforcement. The formation of this discrimination is rarely complete in such instances as the rat's pressing the lever 200 times for a single reinforcement, but the behavior can be acquired and maintained.[8] An aspect of the formation of this discrimination is the spread of secondary reinforcement to the earlier non-reinforced responses in the sequence.

In 1943 Sears (56) set forth a definition of instrumental act that resembles Skinner's earlier formulation in a general way. Sears indicates the difficulty involved in defining "a response" and suggests that when more than one bit of behavior is needed to produce reinforcement, the whole unit should be treated as a response. He specifically points out that more single responses are to be expected in extinction after conditioning where a pair of responses followed by reinforcement is the unit as contrasted with the situation where only single responses are rewarded in training.

Hull (26) does not present a detailed treatment of the problem of partial reinforcement in his book, but in a memorandum to his seminar in the spring of 1941 (25) he discusses what he calls the "Humphreys'

[8] Questions of the mediation of this kind of activity in terms of verbal behavior are obvious and need not be treated here in detail. With practice the human organism can presumably learn behavior in which exceedingly great blocks or units acquire the properties of single responses. The comptometer operator or typist continues to work his machine for long periods of time involving huge numbers of "single" responses in the absence of obvious external reinforcement. On the basis of verbal cues the following kind of pattern of rewarded (R) and non-rewarded (N) trials, if repeated a number of times, should be learnable: R R N R N N N R R N. Even more complex patterns might well come to serve as response "units." No systematic study seems to be available of the learning of such patterns as response blocks by infra-human organisms.

paradox." His analysis pertains to Humphreys' (**28, 31**) findings that resistance to extinction of conditioned eyeblink and GSR is greater after a treatment of semi-randomized 50% reinforcement in conditioning as opposed to 100% reinforcement. Hull's analysis of these results is as follows. For the partial reinforcement group the reaction is conditioned to the after-effects of non-reinforcement. After the first extinction trial the stimulus compound is more like the one conditioned since it now includes the after-effects of non-reinforcement. Presumably the response tendency will be greater to the entire stimulus compound than to any of the parts. From this it is deduced that the effects of preceding extinction trials act to increase response strength above that of the first stimulus presentation of the extinction series for the 50% reinforcement subjects. For groups continuously reinforced in training, the stimulus compound includes only the after-effects of reinforcement, so that extinction trials are much different from conditioning ones. By these means Hull accounts for the initial rise in Humphreys' 50% extinction curves as well as the overall greater resistance to extinction after partial reinforcement as compared with 100%.

Miller and Dollard (**43**, p. 39) have pointed out that greater resistance to extinction can be expected when, in conditioning, unrewarded behavior occurs and is ultimately followed by reward. Miller (**42**) has recently elaborated this point of view. The general position relates to several of the approaches already discussed and to the generalization decrement theory to be treated.

An experimental test that reflects several of these viewpoints and particularly Skinner's stand on the response-unit explanation is that of Mowrer and Jones (**47**). Following a suggestion of J. S. Brown, the crux of their position is treatment of a set of non-reinforced responses as a unit of behavior which is strengthened by the terminal reinforcement. Their results indicate fewer response *units* in extinction for partially reinforced groups, although more *single bar-depressions* occurred. Mowrer and Jones' explanation is based on an "effort" hypothesis derived from a previous study concerning variations in the force required to depress the bar (**46**). In the present context they argue that the more effortful a response, the more susceptible it is to extinction. According to this reasoning a unit of four responses requires more effort than a unit of one response.

It should be noted that the fixed ratio reinforcement procedure employed by Mowrer and Jones poses a special discrimination in the free-responding situation in which rapid responding is differentially strengthened by reward. The larger the number of responses required per reward under this schedule, the higher the response strength in conditioning. It is an experimental question whether the effort phase of the response-unit analysis holds for other kinds of partial reinforcement. With periodic reinforcement of bar-pressing, for example, slow responding is

differentially rewarded. Yet, greater resistance to extinction may follow.

The other hypothesis invoked to explain these data involves Perin's (**52**) temporal gradient of reinforcement. Mowrer and Jones (**47**) cite and support Hull's (**26**) position that a response more than 30 seconds removed from reward is not strengthened. They indicate that the behavior they studied occurred within this limit. They fail to point out, however, that their fixed ratio procedure was on a very limited scale. Other investigators have employed values 48 times as large in numbers of responses (**62**) and greater values temporally (**36, 37, 60, 62**). Also, instances are not accounted for where, for example, the third run down a runway is reinforced and a 90-second inter-trial interval is used. The acceptance by Mowrer and Jones of the 30-second limit on the temporal gradient of reinforcement as a necessary condition for strengthening a response leaves them in the position of failing to account for a considerable body of partial reinforcement data. The generality of this specific phase of the Mowrer-Jones interpretation is exceedingly limited.

It would seem that some mechanism of the response-unit variety is operating. Otherwise, behavior could not be maintained when reinforcements occur only once in nine minutes or every 192 responses. A temporal gradient restricted to strengthening only behavior occurring not more than 30 seconds before reward is, at best, an incomplete account. A temporal gradient may well be one of the factors interacting with several others, but clearly it cannot explain many of the findings that Mowrer and Jones fail to mention.

An important theoretical account of partial reinforcement, somewhat similar to Hull's treatment of Humphreys' data, has recently been proposed and tested by Sheffield (**59**). Her hypothesis was that the greater resistance to extinction after partial reinforcement is based on the conditioning of the after-effects of non-reinforcement in the stimulus compound during training. In extinction after partial reinforcement, the stimulus situation by way of generalization is more like conditioning than after 100% reinforcement. In extinction, as contrasted with conditioning, the cue pattern is changed greatly for the 100% instance but is reinstated for the partial group. The hypothesis was tested by comparing extinction after distributed and massed training on the assumption that the after-effects of presence or absence of reinforcement in conditioning would be dissipated by spaced training. The results were in striking agreement with the hypothesis: Massed training produced a significant, 30% superiority in resistance to extinction for partial over continuous reinforcement. Distributed training reversed the findings slightly, though not significantly.

Consideration of other partial reinforcement studies in which resistance to extinction was the measure shows almost 100% emphasis on

massed training. The only study yielding (an insignificant) superiority of continuous over partial reinforcement in resistance to extinction is Denny's (**7**), and his data are clearly in line with Sheffield's findings since his training trials were even more widely spaced than her distributed condition.

A study bearing directly on the generalization decrement explanation of the greater resistance to extinction found after partial reinforcement has been performed recently by Sheffield and Temmer (**58**). These investigators compared the acquisition and extinction of a running response under the escape and avoidance procedures. They point out that extinction practically always involves a change in the cue patterns present in training and particularly so in the omission of a noxious stimulus. Escape training does not allow the conditioning of the consequences of failure to respond; avoidance training maximally connects the response to cue patterns associated with failure to respond since these patterns are the only ones that are reinforced 100% of the time. (This position has recently been discussed by Miller, **42**.) The theory predicts greater response strength for the escape procedure in training, but greater resistance to extinction following avoidance conditioning. The data clearly support the theory on both scores.

Although the consequences of the Sheffield-Temmer analysis were tested in the escape-avoidance situation, the approach appears to have wide generality of application to comparisons of partial and 100% reinforcement. It accounts for the partial reinforcement facts in acquisition, performance, and extinction in terms of three central concepts: Presentation of reinforcement strengthens a response, omission of it weakens the response, and a change in the cue pattern is followed by a response decrement by way of generalization. The basic note is that "all partial reinforcement procedures connect the response to cue patterns characteristic of omission of reinforcement, cue patterns which normally contribute—through generalization decrement—to loss of response during extinction after 100% reinforcement. Any procedure which introduces some omission of reinforcement during acquisition should tend to weaken the response during training but prolong extinction of the response" (**58**).

Further support for this general position is found in a study by Grosslight and Child (**18**). They point out that the occurrence of a non-rewarded response in a series of rewarded ones permits the response of persisting after failure to be rewarded and strengthened. Such conditioning cannot occur if a failure does not take place in the reward series. The consequences of this position were tested by providing either one or two failures in a series of 10 trials as compared with a control group that received a reward on every trial. The prediction of differential persistence in extinction favoring the failure groups was borne out by the findings.

In a sense, as has been pointed out (47), the situation may be considered in discrimination learning terms. The difference between conditioning and extinction in the 100% instance provides a simply learned discrimination because of the gross dissimilarity between the cues of the two situations. In the partial case, the discrimination is difficult because the cue patterns in conditioning and extinction are similar, i.e., both involve non-reinforcement.

It seems reasonable to assume that a fourth factor is operating in the acquisition and maintenance of partially reinforced responses, namely, derived reinforcement. Non-reinforced responses prior to the reinforced one acquire reinforcing properties since they are ultimately followed by reward. External stimuli support the behavior by way of a similar mechanism. Denny (7) tested the effect of this variable in acquisition by minimizing derived reinforcement in the external environment. When secondary reinforcement was thus reduced, the 100% subjects learned significantly better than the 50% animals; with it operating, the difference between the two groups was considerably less, although still favoring the 100% rats. Denny's results in extinction are confounded by the action of the spacing variable in training. Furthermore, Sheffield (59) has pointed out that for secondary reinforcement to account for the typically greater post-partial extinction curve, it would have to be assumed that secondary reinforcement is stronger than primary reinforcement.

Restricting derived reinforcement in the internal environment poses a more difficult problem. That is, preventing the proprioceptive and kinesthetic stimulation accompanying the response that may well acquire reinforcing properties during training is more troublesome than the parallel restriction in the external environment. Preventing "sensory feedback" from the response by the use of drugs is a possibility.

The overlap between derived reinforcement and the consequences of non-reinforcement in the partial situation is evident. Many of the internal and external cues associated with previous non-reinforcement are present when a reinforced response occurs. Whether the two are separable is an experimental question.

It is noteworthy that the stimulus-response position accounts for certain apparent anomalies, e.g., the greater response strength with less frequent reward under a fixed ratio schedule. This point follows because one basic assumption of this theory is that reward strengthens whatever behavior brings it about. Just as a rat will learn to take the shorter of two paths to food or the one that involves the lesser delay of reward, so it will learn to press faster when more responses are required to produce reward if only because accidentally rapid responding is differentially strengthened.

Expectancy interpretation. The widest use of the principle of expectancy as applied to partial reinforcement situations has been made in a

series of investigations by Humphreys (**28, 29, 31, 32**). Hilgard (**23**) has recently amplified the interpretation. In the present context the concept seems to be essentially a common-sense one that may be summarized as follows (**58**). Responses are given because the subject expects (or learns to expect) reinforcement to occur. In extinction after 100% reinforcement the response drops out quickly because the shift from 100% reinforcement to 0% reinforcement leads to a rapid change to expectation of no reinforcement. In post-partial extinction, the subject continues to expect reinforcement to occur, so he continues to respond (**59**).

As Sheffield and Temmer (**58**) have indicated, the expectancy point of view can be made consistent with the facts by means of a few additional assumptions. This interpretation is certainly parsimonious and couched in words meaningful to laymen. Expectancy, as a principle, however, has been widely criticized in the partial reinforcement context (**7, 12, 42, 47, 58, 59, 66**). Three general points emerge: (1) lack of rigorous definition; (2) anthropomorphism; and (3) lack of generality.

As regards definition, Humphreys (**28, 29, 31**) merely notes that expectations are not necessarily verbal in nature. He does not provide a working definition that offers opportunity for predictions and further tests. Sheffield (**59**) has criticized Humphreys most systematically on this score. She points out that the term is not unambiguous in Humphreys' own situations. The greater post-partial resistance to extinction is attributed by him to the difficulty of shifting from an expectation of intermittent reinforcement to one of complete non-reinforcement, as compared with ease in shifting from an expectation of 100% reinforcement to one of 0% reinforcement. Sheffield suggests that the argument can be reversed: After becoming accustomed to partial reinforcement it should be easier to change to an expectation of uniform non-reinforcement, but after 100% reinforcement it should be difficult for the subject to believe that no further reinforcement is forthcoming. In this connection she makes the further point that expectancy as invoked by Humphreys was an *ex post facto* conjecture based on an empirical analysis.

Miller (**42**) has recently suggested that the use of the term expectancy "is circularly based upon the type of behavior it is trying to explain rather than upon certain explicitly stated principles." He goes on to indicate the possibility of rigorous reformulation of the concept for deductive purposes. The further point is made that the term as used in the partial reinforcement context seems to involve circular reasoning: Organisms approach expected rewarding effects and avoid expected noxious effects (**58**).

Humphreys uses the expectancy notion to explain the initial rise he found in post-partial extinction curves. It has been pointed out (**59**) that this statement adds nothing to the conceptualization that rein-

stating the stimulus pattern conditioned to a response maximizes the probability of the response's occurrence. Furthermore, in other situations the rise is more prominent after *continuous* reinforcement than after partial (**37, 38, 62**). In this connection, Detambel (**8**) reports a failure to find the initial rise in a situation similar to Humphreys' (**29**) verbal response study.

The possibility of defining expectancies as implicit verbal responses in human subjects has been pointed out (**47, 59**). In these terms they would operate like other cues and there would be no need for the additional concept (**59**). The use of such a notion with infra-human organisms is highly questionable; Humphreys himself does not attempt it (**32**), although he notes the possibility of its non-verbal nature (**29**). The difficulties of avoiding the connotations of "expectation" in everyday usage are great; the anthropomorphic implications are obvious (**58**).

Sheffield (**59**) has questioned whether her massed-distributed data can be explained by the expectancy proponents. In her study, both groups learned equally well. Thus, presumably equal expectancies were set up and operated over both the long and short time intervals. Then why should the spaced training wipe out the effects of partial reinforcement in resistance to extinction? Distributed practice should facilitate the acquisition of expectancies as it aids other kinds of acquisition, and these should produce greater extinction differences between partial and continuous groups. But the data indicate the opposite, as predicted by stimulus-response learning concepts.

Mowrer and Jones (**47**) have also suggested a translation of the notion of expectancy into stimulus-response terms. Their point of reference is discrimination learning. The interpretation has been mentioned previously in this paper. It appears to reduce essentially to the generalization-decrement theory, yielding similar predictions and deductions.

The probability theory of Brunswik's (**3**) should be mentioned in the present context. To the extent that his position involves hypotheses and expectations, the same kind of criticism as given previously appears to apply. It may be noted that a simple probability analysis is similar in some respects to one based on strengthening by reinforcement and weakening by extinction (**24**). Other comments on Brunswik's position have been made in Section IV.

VII. Practical Implications

There remains the problem of pulling together the scattered references in this paper to the practical implications and applications of partial reinforcement.[9] Partial reinforcement is certainly more like the

[9] Here and elsewhere in this paper we are concerned mainly with conditioning of the instrumental variety. Partial reinforcement has not been studied extensively or system-

everyday situation than 100% reinforcement. It is a rare instance in human daily behavior that every response of a specified kind leads to presentation of a reinforcing stimulus. Further, partial reinforcement has the advantage of delaying the effects of satiation. Conditioning can be continued over longer periods with partial as contrasted with 100% reinforcement. The animal trainer uses these techniques. He requires the sequence of tricks to be run through completely or the same act to be repeated several times before feeding the animal. Partial primary reinforcement is not, of course, employed alone in these situations. Maintenance of the behavior leans heavily on derived reinforcement: a pat on the head, a word of praise, etc. The occurrence of this kind of stimulation may be partial or 100%.

The evidence reviewed in the present paper suggests the efficacy of partial reinforcement in everyday life. Learning is not greatly retarded by its use. (It appears, however, to be advantageous to use 100% reinforcement in establishing the desired behavior initially.) The behavior is more than adequately maintained with infrequent reward.[10] Finally, the most striking aspect of the data is the far greater resistance to extinction found after partial reinforcement as compared with continuous. If one desires behavior to be maintained for long periods of time in the absence of externally presented reward, partial reinforcement should be used in training. This generalization appears to hold whether one is interested in having a rat press a bar or in maintaining a child's proper toilet habits or table manners. Implications of the procedure for the practical prediction and control of behavior are obvious.

VIII. Summary and Conclusions

The available literature on partial reinforcement has been reviewed, not merely as a compilation of research facts and findings, but rather as a basis for abstracting the major empirical principles that can be drawn from investigations of this topic. Throughout this article the term "partial reinforcement" has been used to refer to the incidence of reward on less than 100% of the trials or responses. The baseline is 100% or continuous reinforcement.

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atically in the Pavlovian substitutive situation [with the exception of Brogden's (1) work], although every test trial in which the US is omitted produces a case of noncontinuous reinforcement. Most of the research in this area has involved responses of the skeletal musculature rather than behavior innervated by the autonomic nervous system.

[10] Also, it should be noted that if the reward involved is expensive in time or money, partial reinforcement is the more profitable procedure for maintaining behavior.

The history and development of this research area have been touched on, and the various techniques for presenting partial reinforcement have been treated. Problems in design are mentioned. The main sections of the paper deal with the effects of partial reinforcement on acquisition, maintenance of behavior or performance, and resistance to extinction. A theoretical section that treats the available explanations of partial reinforcement is included.

Empirical generalizations following from the data are as follows:

1. *Acquisition.* Response strength is built up somewhat more rapidly under a schedule of 100% reinforcement than under a partial regimen. Differences in learning, however, are not always large, and with prolonged training the ultimate level of acquisition for partially rewarded subjects may approach that for the 100% ones.
2. *Maintenance.* While the behavior in post-acquisition performance is stable in the partial reinforcement situation, it is usually at a lower level than in the 100% instance. Nevertheless, the differences are not always statistically significant and may well be of no great practical consequence.
3. *Resistance to extinction.* The most striking effects of partial reinforcement are apparent in response strength as measured by resistance to extinction. In almost every experiment, large and significant differences in extinction favoring the groups partially reinforced in conditioning over the 100% ones were found. The practical implication of this principle for maintaining behavior is obvious: Administer the reinforcing stimulus in conditioning according to a partial schedule, and the behavior will be maintained for long periods in the absence of external support from primary reward.

Stimulus-response learning theory concepts and those stemming from an expectancy point of view are discussed as explanations of partial reinforcement data. It appears that S-R principles provide a framework accounting for the phenomena in this area.

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THE FACTORIAL ANALYSIS OF ANIMAL BEHAVIOR[1]

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INTRODUCTION

The primary purpose of the factorial approach in the study of behavior is to determine the basic parameters which are common to the interrelated variables of a given domain. Obviously then, it is a matter of simplification, an application of the law of parsimony to complex data whose interrelationships are difficult to comprehend. More specifically, in the case of individual differences, a limited number of abilities or factors are isolated as the determiners of behavior in a given domain. Recalling that a variable may be expressed in standard form, Z_j, (where j equals any variable) and denoting common factors by the notation F_j and their coefficients by a_j, we have the following representation of the preceding statement for three of the total number of variables:[3]

$$Z_1 = a_{11}F_1 + a_{12}F_2 + a_{13}F_3 + \cdots + a_{1m}F_m. \qquad [1]$$

$$Z_2 = a_{21}F_1 + a_{22}F_2 + a_{23}F_3 + \cdots + a_{2m}F_m. \qquad [2]$$

$$Z_3 = a_{31}F_1 + a_{32}F_2 + a_{33}F_3 + \cdots + a_{3m}F_m. \qquad [3]$$

To express the variance of a particular variable, which, it will be recalled, is equal to σ^2, square both sides of [1], sum over the n values of the respective variables, and divide by N, thus obtaining for [1]:

$$\frac{\Sigma Z_1^2}{N} = a_{11}^2\frac{\Sigma F_1^2}{N} + a_{12}^2\frac{\Sigma F_2^2}{N} + a_{13}^2\frac{\Sigma F_3^2}{N} + \cdots + a_{1m}^2\frac{\Sigma F_n^2}{N}. \qquad [4]$$

Now, since the variance of a variable in standard form is equal to unity, equation [4] may be written:

$$\sigma_{z_1}^2 = 1 = a_{11}^2 + a_{12}^2 + a_{13}^2 + \cdots + a_{1m}^2. \qquad [5]$$

Thus, the terms on the right (a_{11}^2, etc.) represent the portions of unit variance which are accounted for by each of the respective common factors (F_1, F_2, F_3).

From this it is obvious that if a given variable were a "pure" meas-

[1] The writer would like to thank Professor K. S. Lashley and Professor L. L. Thurstone for critical reading of this paper in manuscript.

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[3] For the sake of simplicity this treatment is concerned only with orthogonal common factors; the uniqueness of each variable is omitted.

ure of a particular factor, say F_1, the a_{12}^2 and a_{13}^2 etc. values would be equal to zero, and the variance of the variable would be entirely accounted for by one common factor (F_1). In essence, the determination of the factor coefficients (a_1, a_2, $a_3 \cdots + a_m$, i.e., factor loadings) is the basic problem of factor analysis. On the human level, Thurstone has isolated several determiners of mental ability (**50**) to account for individual differences in this domain. In recent years, similar attempts

TABLE I

INTERCORRELATIONS BETWEEN EARLY MEASURES OF LEARNING IN ANIMALS

Experimenter	*Date*	*Animals*	*Tests Used*	*Correlation*
Bagg (4)	1920	90 mice	Simple maze vs. multiple choice	.11
Heron (25)	1922	22 rats	Circular maze vs. problem box	.01 to .09
Hunter and Randolph (32)	1924	23 to 37 rats	T maze, sawdust box, straightaway	−.16 to .18
Tolman & Davis (55)	1924	13 rats	Rectangular maze vs. Carr maze	.16 to .66
Liggett (35)	1925	48 chicks	T maze straightaway	−.64 to .19
Williams (68)	1929	25 rats	Visual discrimination vs. multiple T-maze	.16 and .08
Miles (38)	1930	38 rats	Errors, elevated vs. alley multiple T maze	.50
Tryon (58)	1931	141 rats	17 units vs. 20 unit multiple T maze	.77

have been made in certain areas of animal behavior, although paradoxically, no studies have been made of the higher mental processes in animals. This is undoubtedly at least partially due to the lack of evidence of "higher" mental activity among lower animals until relatively recent years. It is also due to the fact that factor methodology did not attain sufficient dignity until about 1935. In any event, factorial analyses of animal behavior are being carried out, and although the factors thus far isolated need further verification, the writer feels justified in

presenting the work done and in pointing out several reasons why such studies are of crucial importance.

Early Correlation Studies

The earliest attempts to reveal interrelationships of animal performance were carried out between two measures of a specific learning task. Partly because of unreliable and invalid measures, and partly because of inadequate sampling, these early investigations revealed near zero intercorrelations. It was not until the studies of Miles (**38**) and Tryon (**58**) that appreciable relationships were shown to exist. A summary of these early studies is reproduced in Table I.

TABLE II

Intercorrelations of Learning Measures of the Rat (9)

	Multiple T-Maze	*Elevated U-Maze*	*Elevated T-Maze*	*Stone Light Discrimination*
Multiple T-Maze		.63	.53	−.10
Elevated U-Maze			.61	.01
Elevated T-Maze				−.02

TABLE III

Intercorrelations of Learning Measures of the Rat (9)

	Triple Platform Problem Box	*T-Maze*	*Light Discrimination*
Problem Box		−.06	−.05
T-Maze			.01

In the early thirties several multiple variable studies were made, primarily by the Stanford group of animal psychologists. These exploratory investigations were not very extensive in scope, although they were all relevant to learning performance. The essence of these studies is presented in Tables II, III and IV. Although these data show that intercorrelations between maze performances are all positive and significant, and that intercorrelations between other similar measures of learning are positive, it is evident that interrelationships between distinctly different measures of learning tend toward zero. Thus, there is no evidence of what might be called a general learning factor. Comparable findings have been reported by Woodrow (**70, 71**) who was unable to find a general learning factor in human subjects. Furthermore, a study

by Campbell (8) indicates that this same specificity of animal behavior is evident in a comparison of rat behavior in "learning" situations and "reasoning" situations. Campbell's results concerning the relationships between maze performance and reasoning performance are shown in Table V.

TABLE IV

INTERCORRELATIONS OF LEARNING MEASURES OF THE RAT (53)

	Warden U-Maze	*U-Maze Reversed*	*Miles Elevated Maze*	*Miles Maze Reversed*	*Multiple Light Discr.*	*Discr. Reversed*
Warden U-Maze		.55	.47	.33	.03	−.05
U-Maze Reversed			.52	.41	−.03	−.19
Miles Elevated Maze				.51	−.01	−.12
Miles Maze Reversed					.09	.00
Multiple Light Discr.						.52

TABLE V

RELATIONSHIP BETWEEN LEARNING PERFORMANCE AND PERFORMANCE ON THE MAIER REASONING TEST (8)

	Stone Multiple T-Maze	*Warden U-Maze*	*Maier Reasoning*
Stone Multiple T-Maze		.55	−.37 (−.29)
Warden U-Maze			−.22 (−.20)

FACTORIAL STUDIES OF LEARNING

The first frontal attack on the underlying structure of learning in animals was made by Dunlap (11) in 1933. Since most of the experimental work on individual differences has been performed on rats, and this original study was done with chicks, it will not be treated in detail. Briefly, Dunlap submitted 119 chicks to a battery of ten tests, mostly learning tasks. The obtained correlation matrix (average r around .20) was subjected to tetrad analysis to determine whether the underlying

traits could be analyzed into a single general factor plus specifics. Such a pattern was found inadequate and the non-existence of a general learning ability was again confirmed, this time in chicks.

McCulloch (37) was interested in the same problem in rats. He obtained results of the performance of 85 rats on eight learning tasks, all of which involved the "apprehension of relations."[4] The battery consisted of six discrimination tests, one 27 unit elevated maze, and a two-pedal problem box, plus an irrelevant test of motor dexterity. As can be seen in Table VI, except for one *r* of .44 (between two light

TABLE VI

INTERCORRELATIONS OF MEASURES OF LEARNING OF THE RAT (37)

	I	II	III	IV	V	VI	VII	VIII	IX
I									
II	44								
III	11	09							
IV	03	11	20						
V	04	−05	11	−30					
VI	−16	−09	−16	−18	01				
VII	−04	−24	13	15	11	08			
VIII	−04	−04	−01	08	08	−11	−27		
IX	−14	−22	11	−05	06	19	15	−18	

discrimination tasks), the average *r* is around .016. Obviously, the magnitude of the intercorrelations precludes the existence of a "*g*" factor to be extracted from these data.

Robert Thorndike made the first comprehensive investigation of rat behavior which involved an application of the Thurstone centroid method of factor analysis. His purpose was to cover as wide a variety of learning problems as possible. For this rather extensive task he used seven experimental set-ups from which he derived 32 scores (average inter *r* of .18). The apparatus were as follows: activity wheel, Warner-Warden maze (two patterns), elevated T maze (two patterns), Jenkins circular problem box, latch problem box, Warner conditioned response test (two stimuli), and the Columbia obstruction apparatus. Several analyses were made, one using the Hotelling solution, one the Thurstone solution. Also, analyses were made on three different matrices, one of 13 variables, one of 20 variables, and one of 32 variables. Although the loadings differed in detail, the basic patterns were the same. It was concluded that three factors accounted for the total variance to be analyzed. These Thorndike (49) labeled as follows: docility (common to all

[4] What Spearman in his two-factor theory has referred to as eduction.

tests), transfer (distinguishing early from later tests), and a conditioned response factor (specific to conditioned response scores).

In the opinion of several investigators, Thorndike's results were not regarded as particularly meaningful. It was pointed out that although Thorndike used the centroid method, he did not rotate his obtained factor loadings to test for simple structure, and hence, to reach a more meaningful solution. As a consequence, Thorndike's data were re-analyzed several years later by Van Steenberg (**63**). In methodology, the essential difference in this study is the rotation of axes to a more meaningful configuration. In this analysis, ten factors were isolated, only five of which were interpreted. These five, which are primarily relevant to learning, have been labeled as follows: visual (elevated mazes), adaptability to test situations, speed, kinesthetic ability, and visual insight (maze Gestalt).

These investigations were followed by a similar study by Vaughn (**64**) in 1937. Judging by the battery of tests employed, the purpose of the investigator was to explore the entire rat behavior domain by use of tests of known reliability and validity. The ten pieces of apparatus employed were: wildness tunnel, activity cage, straightaway, simple maze, multiple T-maze, rectangular maze, problem box, Maier "reasoning" test, and a perseverance box. In the factorial analysis of the 34 variables obtained, the investigator in this experiment employed the centroid method with rotation to test for simple structure. Of the eight factors isolated, four were sufficiently clear to be interpreted. The labels were given as follows: speed, wildness-timidity (docility), insight learning, and transfer. Again, the essential nature of the factors obtained, with the possible exception of wildness-timidity, refer to the organization of learning behavior.

A comparison of the several studies thus far reported indicates several trends which will perhaps be made clear by the summary presented in Table VII. These data, coupled with the indices of earlier correlation studies lead to several fairly clear relations. The most obvious conclusion is that the assumption of a unitary learning factor is untenable. This is most clearly indicated in the factorial studies of Dunlap (**11**) and McCulloch (**37**); particularly in McCulloch's investigation which was specifically directed at Spearman's original postulation of "g." An alternative explanation is that several factors are involved in the organization of learning. The subsequent studies of R. L. Thorndike (**49**), Van Steenberg (**63**), and Vaughn (**64**) attest to this interpretation, although the exact nature of such group factors is not certain at present.

The data presented thus far have all been concerned with learning performance regarded as a static affair. Wherry (**65, 66, 67**) has initiated a unique series of studies which gets at the dynamics of learning by clever usage of the factorial methods. In the first place, Wherry claims that a mere analysis of composite scores of behavior for an entire learn-

ing situation will not give a complete picture of what goes on. He postulates the idea that many simultaneous learning processes are taking place at the same time within a given task. Thus, each of these may have learning mechanisms which differ from one another. For example, it may be possible that a certain factor which accounts for performance for a few trials when it is dominant may be relatively unimportant in a later part of the learning situation when another factor dominates. It is this very hypothesis which his studies were carried

TABLE VII

SUMMARY OF FACTORS ISOLATED IN RAT LEARNING DOMAIN

No. of Factors Isolated	*Identification of Factors*	*Investigator*	*Year*
0	no "*g*"	McCulloch (37)	1935
3	Docility Transfer CR factor	R. Thorndike (49)	1935
5	Visual Adaptability Speed Kinesthetic Visual insight	Van Steenberg (63)	1937
4	Speed Docility Insight learning Transfer	Vaughn (64)	1937

out to test. In order to get at the entire learning process, his analyses were applied to correlation coefficients obtained between the separate trials or stages of the learning period (i.e. period 1 correlated with period 2, period 3; period 2 correlated with period 3, period 4, etc.). Thus, various alleys were considered as individuals, and the number of entrances into each alley was considered as the score for that day, trial, or period. The intercorrelation matrix obtained in this fashion was then tested by the centroid method and rotated for simple structure. This procedure was carried out on five sets of experimental learning data which had been gathered by other investigators. The data include 2 learning experiments described by Yoshioka (72, 73)—pattern discrimination and distance discrimination—Hunter's (30) simple and complex

maze performance, Ruch's (47) linear mazes, and Tryon's (58) 17 unit multiple-T maze for both bright and dull rats. In my estimation the results are quite striking. Furthermore, they are consistent! Exactly the same pattern was found regardless of the particular learning problem confronting the animals. Samples of these typical results are reproduced in Figs. 1 and 2. At this stage of the knowledge of such factors

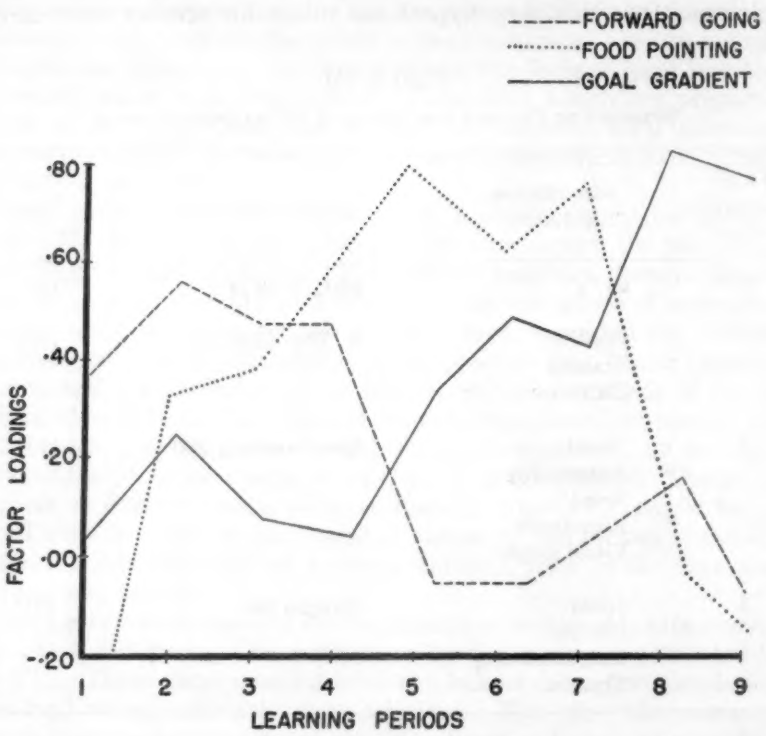

FIG. 1. FACTORS OBTAINED FROM YOSHIOKA DISCRIMINATION DATA (65, p. 267).

it would appear that the specific description of the factors obtained would depend upon the nature of the problem confronting the animal. In any event, Wherry (65) has offered a general description which adequately accounts for the factors obtained regardless of its specific setting. There are inferences that the initial factor corresponds to trial and error running, that the rising and waning factor involves retracing and uncertain behavior, and that the last factor represents the insightful learning of the correct response. However, Wherry (pp. 271–272) cautiously makes only the following conclusions: (1) These preliminary studies have shown that factors present in learning situations are not

static affairs, acting uniformly throughout the learning of the maze. (2) Furthermore, it is possible by means of proper factorial analysis to separate the effects of these various factors, thus enabling us to see the course of their rise and wane as well as giving us a less confusing picture of their importance.

In another paper, Wherry (67) analyzes the important genetic studies of individual differences in rat maze behavior which Tryon has been conducting over a period of about twenty years. Wherry offers

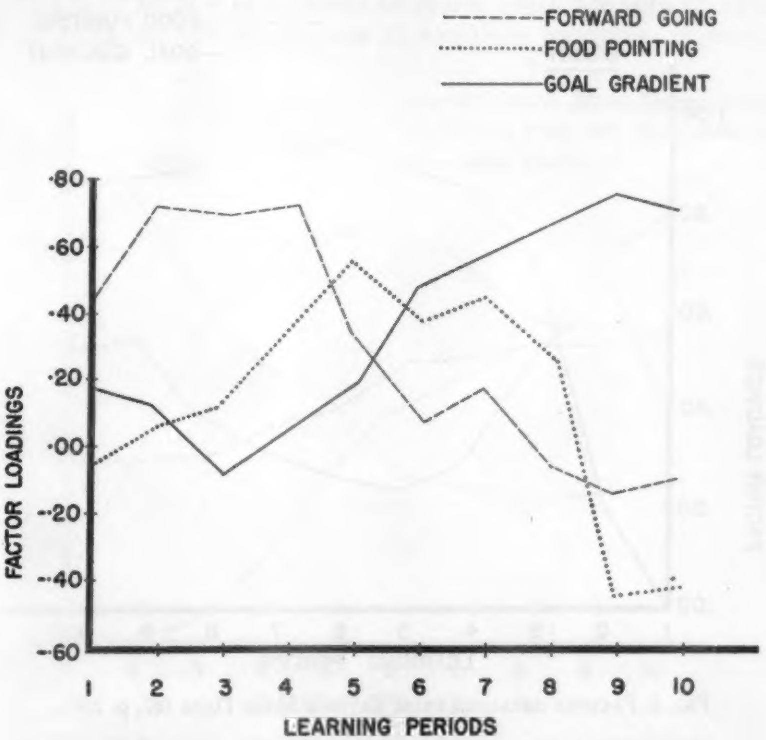

FIG. 2. FACTORS OBTAINED FROM HUNTER MAZE DATA (65, p. 268).

a more parsimonious description of the ten components suggested by Tryon (60) and then goes on to an analysis of the differential effects of the obtained factors in the learning situation. The results obtained, again, are consistent with results previously obtained by Wherry. This fact is immediately obvious by inspection of Figs. 3 and 4. In addition to pointing up the consistency of Wherry's results, these data are of interest on several other counts. In the first place, it will be recalled that the animals u3ed in Tryon's original studies were selectively bred

for several generations for brightness and dullness in maze performance. Furthermore, the number of cases used in each category is quite large: 550 bright and 550 dull. Now, it will be noticed in Figs. 3 and 4 that, although the results are similar in pattern, there are differences in the specific factor loadings which are attributable to genetic differences. Thus, by inspection of these data it is obvious that the dull animals make greater use of the food-pointing factor and less use of the forward-

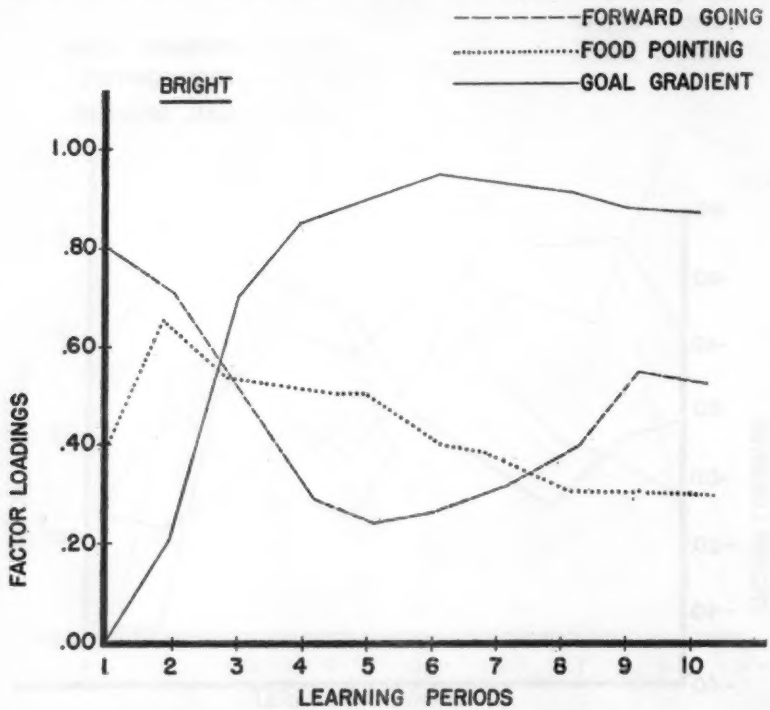

FIG. 3. FACTORS OBTAINED FROM TRYON'S MAZE DATA (67, p. 239).

going factor than do the bright animals. What is perhaps a little more obvious is that the third factor (labeled "goal gradient"), which may be thought of as "insight," appears quite strongly as early as the third period of learning, whereas the dull animals employ this factor only gradually and not heavily until about the seventh period of learning. Wherry attempts a tieup with Krechevsky in an explanation of this particular aspect of the data:

> The results seem to be in agreement with Krechevsky's finding that the bright rats tended to make greater use of spatial (interoceptively released) hypotheses, while the dull rats were more apt to use visual (exteroceptively re-

leased) hypotheses. This interpretation was supported by the relative contributions of forward-going tendency (interoceptive) and food pointing (exteroceptive) factors in the two groups. (67, p. 251).

Studies of Emotionality

Investigations similar to those reported in the learning area have been conducted in two other domains of rat behavior, namely, emotionality and motivation. Studies in these areas, however, are severely handicapped by lack of sufficient measures which are both reliable and valid. Despite this meagreness of available measures, investigators

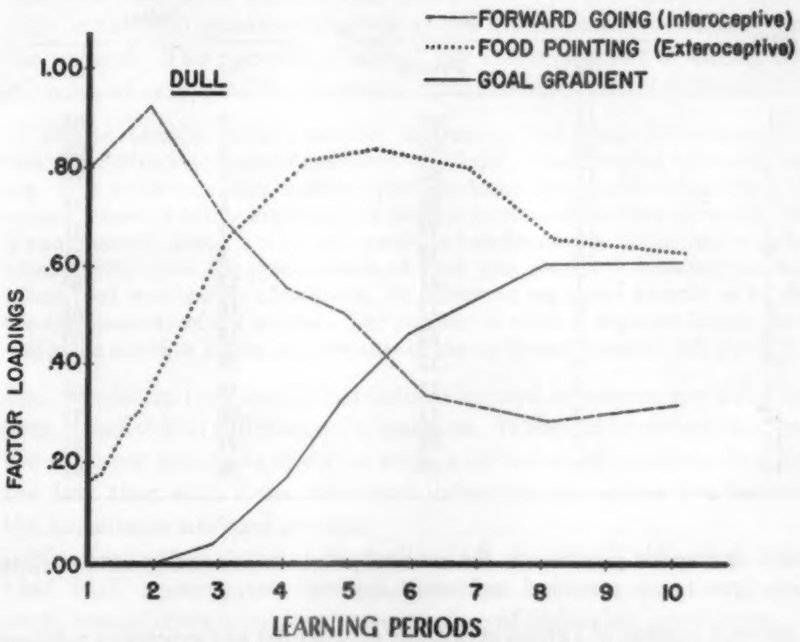

Fig. 4. Factors obtained from Tryon's Maze Data (67, p. 239).

have pushed on with whatever means were available, and at least one factorial investigation relevant to each of these areas has been reported within the past ten years. Once again, however, such investigations were dependent upon previous groundwork in the way of adequate measures of emotionality and motivation as well as certain preliminary interrelationship studies. These will be treated briefly by way of leading up to the factorial investigations.

In the emotionality area, practically all of the work has been car-

ried out by Hall and his associates. Work in this area has obviously been slow in getting started because of the great difficulty in establishing a useful index of emotionality. However, in 1934, Hall (13) reported an index of emotionality which has apparently withstood the test of time and criticism. Because of its importance with respect to later studies, a brief treatment of this investigation will be given. Hall begins

TABLE VIII

NUMBER AND PERCENTAGE OF RATS DEFECATING AND EATING FOR EACH TRIAL (13, p. 388)

Days	Defecating		Eating	
	No.	*Percent*	*No.*	*Percent*
1	18	69	2	08
2	18	69	2	08
3	17	66	5	19
4	10	38	7	27
5	13	50	8	31
6	10	38	8	31
7	7	27	13	50
8	6	23	16	61
9	4	15	18	69
10	4	15	20	77
11	3	11	18	69
12	3	11	20	77
13	2	08	22	84
14	2	08	24	92

by explaining his preference for a concept of emotionality rather than emotion.

This state consists of a group of organic, experiential and expressive reactions and denotes a general upset or excited condition of the animal. Emotionality can be thought of as a trait since animals and men differ in the intensity of emotional reactions displayed. The reader is warned against interpreting emotionality as a thing or faculty. It is merely a convenient concept for describing a complex of factors. Emotionality is preferable to the term emotion since the latter implies for most psychologists that a differentiation of emotions can be made. The current point of view is that such differentiation is extremely speculative (13, p. 385).

With this conception in mind, Hall proceeds to establish his measure for individual differences in emotionality among rats. He placed 31 rats in a strange "open field" for three minutes a day for twenty days.

Lettuce was scattered within the enclosure, and the rats were appropriately motivated. Defecation, urination, and eating were recorded. The following highly interesting results were obtained (Table VIII). It is obvious from this table that there is a significant relationship between emotional defecation and eating. Initially the percentage of animals defecating is high (69%) and the percentage of animals eating is low (8%). As the percentage of animals defecating decreases, the percentage of animals eating increases. Expressed numerically, Hall reports a correlation of .82 between "days defecating" vs. "days not eating." A similar relationship obtains between "days urinating" vs. "days not eating" (*r* equals .70). The raison d'être for the rats' emotionality in the field situation is given as the unfamiliarity or strangeness to the animal. The reasoning behind the establishment of eating as the criterion of emotionality is succinctly stated by Hall as follows:

> The problem of finding another measure of individual differences in emotionality (criterion) was not easy, and was finally solved by the following reasoning. It is a commonplace that an upset condition hinders the adjustment of the animal. Emotionality may act as a barrier to the satisfaction of needs. Hence it was reasoned that if a rat which would eat under normal living cage conditions immediately upon the presentation of food was placed in a strange enclosure, where food was readily obtainable, its failure to eat could be said to be due to the emotionality of the animal. The number of trials it required before the food was eaten could be taken as a measure of the rat's emotionality (**13**, pp. 389–90).

Hall concludes that emotional defecation and urination are valid measures of individual differences in emotion. It should be noted that one of the stronger points in favor of such a criterion of emotionality lies in the fact that such defecation and urination are under the control of the autonomic nervous system.

In a subsequent series of studies (**14, 15, 16, 17, 18**) from 1934 to 1941 Hall investigated interrelationships between emotionality and need, ambulatory activity, stereotyping of behavior, persistence, and behavior derangements. His primary conclusions are that emotional disturbance tends to be reduced when the rat is impelled by a strong need, emotional rats are less active than relatively non-emotional rats, the emotional rat is less stereotyped (more variable) in his behavior, emotional rats are less susceptible to audiogenic attacks than the non-emotional rats, and that persistence and emotionality are not related temperamental traits. In a later study (**23**), in which genetically derived strains of emotional vs. non-emotional strains were selectively bred, Hall provides evidence for the inheritance of aggressiveness in the non-emotional strain and timidity in the emotional strain.

These studies culminated in an intercorrelational study by Billings-

lea (5) in 1941. In this study the investigator studied the relationship between emotionality and various other allied indices. The measures included Hall's open field situation, an activity wheel, a modified Vaughn paper barrier (persistence), coordination of paws and mouth in an enclosed food dish, aggressiveness, wildness-timidity, and the Martin-Hall standard situation for measuring abnormal behavior. The subjects were 40 adult female rats, 20 from Hall's emotional strain and 20 from the non-emotional strain. In a later paper (7), Billingslea subjected the obtained intercorrelation matrix to a factorial analysis. Three factors were postulated, and identified as emotionality, freezing, and timidity. The factor loadings are reproduced in Table IX. The largest factor loading for each factor is marked with an asterisk to indicate the essence of the factors identified. It is obvious that similarly high loadings vary in the same direction. Although much remains to be done, the results are encouragingly clear and meaningful.

TABLE IX

FACTOR COEFFICIENTS OF EMOTIONALITY FACTORS (6, p. 209)

Variables	*Emotionality*	*Freezing*	*Timidity*
Emotionality	.616*	.348	−.227
Activity	.449	−.271	−.358
Paw-mouth	−.017	−.161	−.030
Fighting	−.008	−.040	−.619*
Probl. Solving (persistence)	−.082	.652*	.433
Aggressiveness (air stream)	−.389	−.502	.106
Wire T. (Time in start cage)	.450	.023	.279
Wire tunnel (time in tunnel)	.403	.141	.398
Pipe (S. C. time)	−.026	.528	.553
Pipe Tunnel (time)	.023	.407	.382
Behavior Disturbance	−.003	−.535	.033

STUDIES OF MOTIVATION

With the exception of one study by Rethlingshafer (43) all large scale intercorrelational studies of motivation and drives reported thus far have been conducted by Anderson (1, 2, 3). The most thorough of the three interrelated studies involves 47 measures obtained from 14 experimental set-ups. Without specifying the apparatus employed, the measures obtained may be summarized as follows: exploration, 7; hunger, 14; thirst, 10; sex, 7; learning, 6; and one measure each of body weight, activity, and emotion. Although Anderson had originally intended to carry through with a factorial analysis of the obtained inter-

correlation matrix, he did not perform such an analysis. His attitude on the necessity for such a procedure is perhaps indicated in his first paper wherein he states that "methods of factor analysis have been developed in an effort to determine just what factors may be involved in any given test, but at the present stage of development of the methods (1934) the analyses are often more confusing than illuminating because the factors fail to make good psychological sense" (1, p. 107). Although there was some justification for this point of view when factor theory was in its early stages, there are indications that these same data could well be analyzed factorially by current methods in an effort to obtain greater clarity of the underlying relationships. This is particularly true in areas where Anderson's interpretations are admittedly speculative. One outstanding instance of this is apparent in the following conclusion wherein he states: "There is a fairly general tendency for sex tests, learning measures and the emotional defecation test to yield significant or almost significant intercorrelations. There is thus some evidence of the operation of a general factor or factors within these groups of tests" (2, p. 113). In any event, certain generalizations were apparent from the data; they may be briefly summarized as follows:

1. Exploratory tests correlated significantly with other exploratory tests.
2. Sex tests correlated significantly with other sex tests.
3. Thirst tests did not correlate significantly with other thirst tests.
4. Hunger tests did not correlate significantly with other hunger tests.
5. Learning measures correlated with other learning measures.
6. No general drive factor is in evidence between measures of different drives.
7. Sex tests, learning measures, and emotional defecation are interrelated.
8. Learning measures show little tendency to correlate with motivational measures.

Further generalizations which need to be confirmed by factorial analysis are: "The data obtained indicate that the performance on any given test may be a complex function of a number of factors, many of which are probably quite independent of the particular drive involved" (2, p. 98). And "one is not justified in assuming that any given drive is necessarily a unitary factor operating to produce significant and consistent intercorrelations among different tests of the specified drive" (2, p. 107). The only safe generalization that can be made at this point is that the different drives are independent and are not related to learning ability.

One other multiple variable investigation in the motivation area has been reported by Rethlingshafer (43). In essence, this study is an ap-

plication of the Wherry procedure of applying factor analysis to periodic learning. In this instance the investigator was concerned with differences in the course of learning under varying motivating conditions in the learning of a visual discrimination habit. The original experimental data were gathered by Muenzinger (39) who used two sets of motivating conditions: (a) producers of fast learning: shock-wrong, shock-right, shock-after-choice, jump-after-choice, and delay-at-the-point-of-choice;

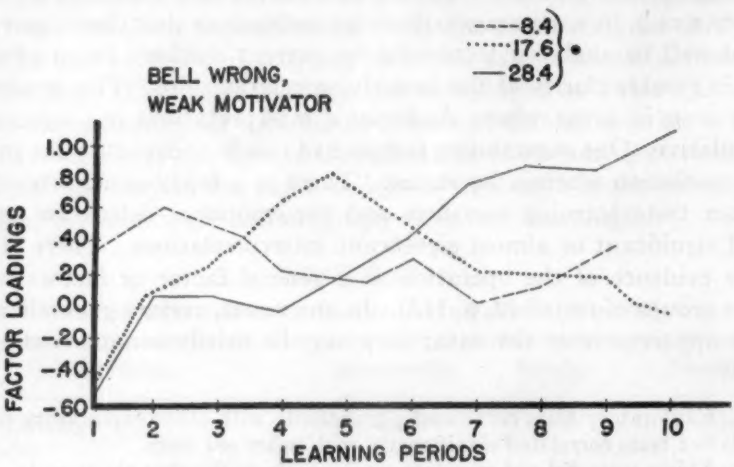

FIG. 5. FACTORS OBTAINED FROM RETHLINGSHAFER'S LIGHT-DARK DISCRIMINATION DATA (43, p. 586).

* Represents the percentage of variance for which each factor is accountable.

(b) producers of less rapid learning: shock-before-choice, food only, jump-before-choice, buzzer-wrong, and buzzer-right. Rethlingshafer employed eight of these motivating conditions, and, as might be expected, obtained the typical three-factor pattern reported by Wherry. Although the patterns are similar, the effectiveness of a given factor does vary under different motivating conditions. This becomes quite apparent from analysis of some of the data reproduced in Figs. 5, 6, and 7. For example, in Fig. 6 where the motivation is food only, no shock, the middle factor is non-operative. On the other hand, this same factor dominates in the rat's learning performance where the motivation is shock-right. Notice in particular the unusually high proportion of the total variance (40.8) which is accounted for by this factor as opposed to zero for performance represented in Fig. 6 and 17.6 in Fig. 5. Such differences are particularly provocative when it is pointed out that the motivator operative in the performance of Fig. 7 was proposed by

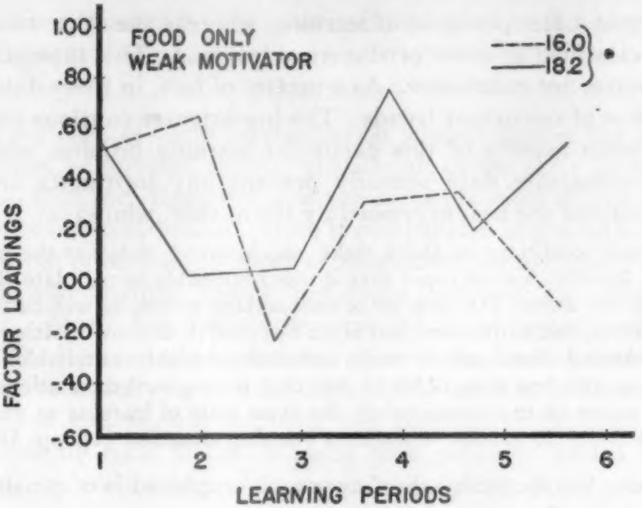

FIG. 6. FACTORS OBTAINED FROM RETHLINGSHAFER'S LIGHT-DARK DISCRIMINATION DATA (43, p. 586).

* Represents the percentage of variance for which each factor is accountable.

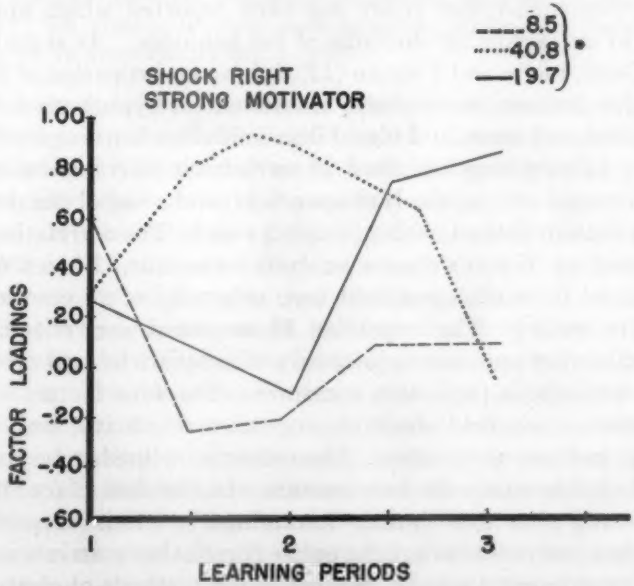

FIG. 7. FACTORS OBTAINED FROM RETHLINGSHAFER'S LIGHT-DARK DISCRIMINATION DATA (43, p. 586).

* Represents the percentage of variance for which each factor is accountable.

Muenzinger as a fast producer of learning, whereas the other two motivators are classified as lesser producers of learning. This type of analysis is, of course, not conclusive. As a matter of fact, in these data there are no indices of consistent trends. The investigator cautions that certain unforeseen aspects of this particular learning problem which resulted in inadequate data actually prevent any legitimate analysis. These limitations are best expressed by the author, who says:

> Under such conditions as shock-right, shock-wrong, delay-at-the-point-of-choice, etc., learning was so rapid that it was impossible to correlate the error scores for all ten days. The number of rats making errors, as well as the total number of errors, had so declined that often by the fifth day the resulting narrow range and skewed distributions made correlations highly unreliable. Hence some matrices were less than 10 by 10, but they represented correlations among day's error scores up to approximately the same state of learning as was represented by a 10 by 10 matrix in a slower learning situation (**43**, pp. 584–5).

Nevertheless, the methodological approach employed is of considerable interest and significance.

Emotionality-Learning Study

In addition to the studies reported in the fields of learning, emotionality and motivation, one study has been reported which apparently attempts to cut across all domains of rat behavior. As stated by the authors, Geier, Levin and Tolman (**12**), it is an investigation of the interrelationships between measures of emotionality, hypothesis formation, vicarious trial and error, and visual discrimination learning in the white rat. Using 57 rats, they obtained 29 tetrachoric intercorrelations from two experimental setups, the Hall open field, and a visual discrimination apparatus similar to the Lashley jumping stand. The correlation matrix was analyzed by Tryon's cluster analysis technique. Eleven measures were obtained from the open field test, primarily of an emotional and adaptability nature. The remaining 18 measures were obtained from the discrimination apparatus, primarily vicarious trial and error measures, and hypothesis formation measures. The four factors identified were labelled: open field timidity, cognitive reactivity, unsystematic variability, and non-motivation. The writer is inclined to be particularly critical of this study for two reasons. In the first place, the areas under investigation are "under-determined." This is particularly obvious when one realizes that the entire correlation matrix was derived from two experimental setups. Secondly, the methods of cluster analysis are not as consistent, nor do they result in as psychologically meaningful results as the more stable methods of multiple factor analysis. In essence, of all the studies presented in this survey, the last investigation

reviewed is theoretically the most inclusive and methodologically the least adequate.

Discussion

The criticism of underdetermination leveled against the last study can perhaps be directed against virually all of the early investigations presented thus far. Basically, it is the same criticism offered by Wolfle who points out that "the situation is comparable to giving human subjects a dozen or so tests of speed of movement, temperament and emotionality, and learning ability, and then analyzing the resulting correlations. Many low correlations would be expected—and found" (**69**, p. 35). Perhaps what is needed most at the present itme is greater stress on the underlying relationships within a given behavior domain. Then, when the several domains are adequately explored, a giant study covering each of the domains with properly loaded variables, might well be undertaken. Tryon goes so far as to state that

> the scientific study of individual differences is characterized more by the prospects of the future than by a collection of conclusive data . . . in the rat, the major problems of individual differences have been attacked in only a few domains, and many other gross domains of behavior (sense acuities, insight formation, delayed responses, conditioned responses, etc.) remain relatively unexplored. . . . We grievously need more complete studies on more behavior domains in many more species (**62**, p. 360).

If several meaningful parameters can be identified from such batteries, more important studies of the genetic constitution and neurologic functioning of animal "Primary Mental Abilities" could follow. Several well known investigators have expressed considerable regard for such approaches. Wolfle states that "the factorial analysis of ability in animals is still practically untouched as a problem for research. Since animals have many advantages over human subjects for use in future studies of the origin, development, and inheritance of factors of ability, a more thorough knowledge of the factors involved in their ability promises to be of considerable research importance" (**69**, p. 36). More specifically, Price states that "however much the factorist has oversimplified his case in relation to genetical structure, it should be granted that there probably exist genetical sources of intercorrelation in some degree similar to so-called general and group factors" (**41**, p. 184).

On the neurological side, Lashley makes some very pertinent remarks in a paper on the "Coalescence of Neurology and Psychology." So much of what Lashley has to say in this paper is pertinent here that the writer is tempted to include several pages of quoted material. However, the essential relationships between the isolation of functional

factors and neurologic functioning is pithily brought out in the following two paragraphs:

This method is discovering functional variables which are not those of classical psychology. For example, it distinguishes between the ability to think in terms of the spatial relations of objects and to comprehend non-spatial relations. It distinguishes facility in manipulating separate symbols, as in recognition of words in jumbled letters, from facility with combinations of symbols as in grammatical speech. It is difficult to describe these variables except in terms of the tests from which they are derived, for they do not correspond to any familiar classification of functions. They do seem, however, *to correspond to functions which may be independently lost as a result of localized brain injury.*[5] Certain types of apraxia are marked by difficulty in dealing with spatial relations; the function represented by manipulation of isolated symbols resembles the ability which suffers in verbal aphasia as defined by Henry Head, and there are other less clear correspondences.

Psychology has still to discover how the various factors revealed by such analysis interplay to produce organized thought. Neurology likewise still has much to do in the investigation of the interaction of cortical fields which are associated with diverse functions. Nevertheless the discovery that the various capacities which independently contribute to intellectual performance do correspond to the spatial distribution of cerebral mechanisms represents a step toward the recognition of similar organization in neurological and mental events (**34**, pp. 468–469).

The eventual localization of factors within the central nervous system thus becomes a plausible task. Obviously, by controlled destruction of varying areas and varying amounts of the brain and the consequent changes in the factor loadings, perhaps to the extent of complete extinction, important observations can be made relevant to the neurologic nature of such factors. The problem is relatively clear cut, but the first step remains to be accomplished in animal behavior—namely, the identification of stable, meaningful psychological factors.[6]

In the past, critics of factor theory have pointed out that factors are, at most, convenient classificatory conceptualizations. In fact, it has been stated that the nature of a given factor is such that it changes when moved from one battery of variables to another and that this is rather conclusive indication of the mathematically artifactual nature of factors. It has also been pointed out that the number of possible factorial solutions to a given correlation matrix is infinite. In order to meet these criticisms, Professor Thurstone introduced the concept of "simple structure," which assumes that "mind is not a structureless

[5] Italics mine.

[6] Studies of this type which are aimed at the analysis of hereditary and environmental mechanisms affecting dog behavior are now under way in the Division of Behavior Studies, Roscoe B. Jackson Memorial Laboratory (see **44, 45, 46**).

mass, but that it is structured into constellations or groupings of processes that can be identified as distinct functions in the test performances" (**51**, pp. 193–194). Although an hypothesis, this is perhaps the most important conception in the development of factorial analysis as a general scientific methodology. This principle provides a guide in the rotation of arbitrary axes to what Thurstone has called a "psychologically meaningful" solution. Perhaps the most striking thing about this procedure is that it leads to an invariance in the isolation of factors. This phenomenon is not to be taken lightly, since it does not seem reasonable that an event of this sort could occur by chance. The stable factors identified by the rotating principle of simple structure have been referred to as "functional unities," the implication being that these may reflect separate processes which are mediated by the genes, the nervous system, the endocrines, the visual mechanisms, the culture, etc., depending upon the domain under investigation. In particular, it seems reasonable to suspect that "functional unities" of mental ability are a function of the nervous system and its metabolism, and that "functional unities" of emotionality are a function of the physiology of the body as well as conditioning, with genetic mechanisms imposing limits of variability in both domains. The primary contention of this article is that these notions need experimental verification, and that factors involving organismic processes can best be experimentally investigated using animal populations. It is difficult for the writer to see how factor theory can go beyond the behavioral isolation of theoretical parameters (intervening variables, if you wish) as long as it confines itself to human populations. The fact that the results reviewed in this paper have not yet given answers to questions of this type is not an indication of the inappropriateness of factor methodology, but rather reflects insufficient and inadequate utilization of the methods. The point is that it will take a relentless and long-range attack in order to give this approach an apportunity to stand or fall under experimental inquiry. One cannot demonstrate factorial invariance with one factorial study of a given domain any more than one can identify meaningful factors in a battery of ill-conceived variables.

The writer, then, would defend the factorial approach as the most powerful methodology available on the behavioral level for the unravelling of the ever present mystery of what a given test measures. Comparative psychology cannot afford to ignore the issue of test validity, nor can factor theory afford to stop with the mere identification of factors. A wedding of the two approaches should strengthen each by leading beyond the isolation of unifying concepts to the detailed description of underlying mechanisms of behavior.

Summary

This paper is concerned with the theoretical and experimental implications of animal behavior studies carried out within the framework of factor theory. The writer has reviewed all the known factorial studies of animal behavior, as well as relevant large scale correlation studies. Since only a beginning has been made in each of the areas of learning, emotionality, and motivation, no reliable conclusions can be drawn as to the nature of the psychological factors involved in any of these domains. However, all of the analyses have been consistent in denying the existence of a unitary factor and in pointing up the existence of multiple factors.

The writer has stressed the importance of pursuing these early leads further because of the necessity for verifying experimentally the theoretical concept of "simple structure" on an organismic level. It was also pointed out that the theoretical structure of comparative and phsyiological psychology would be greatly strengthened by a method which could provide insight into the nature of the basic variables involved in its experimental and test situations.

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A NOTE ON THE USE OF THE METHOD OF SUCCESSIVE DIFFERENCES IN EMPIRICAL CURVE FITTING

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Guilford (1) and later Lewis (2) present a technique called the "Method of Successive Differences" for determining the constants in an equation representing a straight-line relation for a given set of data. The technique essentially involves taking successive pairs of points to get several estimates of the slope constant, and using the mean of these several estimates for the slope constant. Once the slope constant has been determined, the intercept constant is determined by substituting the mean of the X and Y values into the equation and solving for the intercept constant. The purpose of this note is to point out that this method is neither legitimate nor meaningful, and should be discarded from text-book consideration.

Mathematically, the problem is to determine the values of a and b for a given set of data in the following equation:

$$Y = a + bX. \tag{1}$$

With the method of successive differences, the value of b is determined first, as follows:

$$b = \frac{\dfrac{Y_1 - Y_2}{X_1 - X_2} + \dfrac{Y_2 - Y_3}{X_2 - X_3} + \dfrac{Y_3 - Y_4}{X_3 - X_4} \cdots + \dfrac{Y_{n-1} - Y_n}{X_{n-1} - X_n}}{n - 1}. \tag{2}$$

In many cases the values for X are equally spaced, which means

$$X_1 - X_2 = X_2 - X_3 = X_{n-1} - X_n.$$

Substituting in [2]

$$b = \frac{Y_1 - Y_2 + Y_2 - Y_3 + Y_3 - Y_4 \cdots + Y_{n-1} - Y_n}{(n - 1)(X_1 - X_2)}$$

$$= \frac{Y_1 - Y_n}{X_1 - X_n}. \tag{3}$$

Thus when the values of X are equally spaced, the slope constant is completely and solely determined by two points—the first and the last. The example given by Lewis illustrates this point perfectly. Regardless

of what happens to the points in between, as long as the first and last points remain the same, the slope of the "best-fitting" curve will be the same. As pointed out above, the value of a in equation [1] is found by substituting the mean values of X and Y into the equation with the already determined value of b. With this method, it is easy to arrive at some peculiar fits, to say the least.

If the values of X are not equally spaced, then the value of the slope constant is not determined completely by the end values, but to a more or less degree depending on how equally the X-values are spaced. However, in the determination of a slope constant, the weighting of each successive slope estimate should be proportional to the distance along the X-axis. In other words, the calculation should take into account at least the first moment in the weights, or some rather peculiar results can occur. Fig. 1, for example, illustrates how the slope of a straight

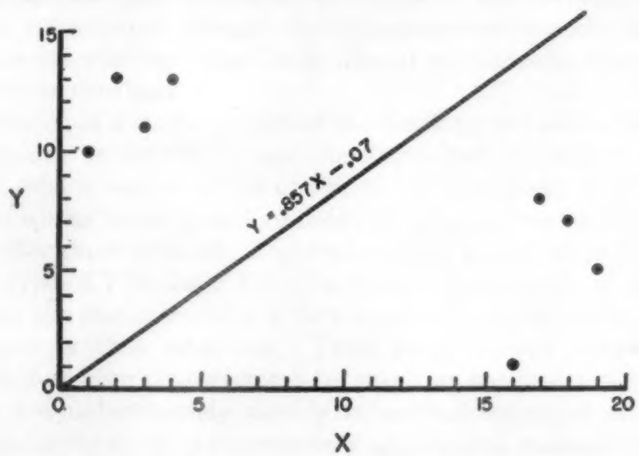

FIG. 1. AN ILLUSTRATION OF A "BEST-FITTING" CURVE DETERMINED BY THE METHOD OF SUCCESSIVE DIFFERENCES.

The straight line shown (and its equation) is the one which best fits the eight plotted points, when the best fit is determined with the method of successive differences.

line can be completely wrong if no account is made of relative distance along the X-axis.

We have the difficulty then that an erroneous slope can be computed if each slope estimate is not weighted in proportion to the distance along the X-axis. On the other hand, if equation [2] is changed to weight each value correctly, then equation [2] again reduces to equation [3], and once again the slope is determined solely by the two end points.

It is to be hoped that this method will not be perpetuated in future texts.

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FOUR STUDIES IN PSYCHOLOGY AND SOCIAL STATUS

A SPECIAL REVIEW[1]

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American social scientists in the last two decades have begun to concern themselves increasingly with the social and psychological concomitants of the division of labor in society and the multifarious inequalities that either accompany or stem from it. The commentary and theoretical works of an earlier day, which had only the most meagre of underpinning in direct observation and unchallenged fact, have now given way to the research report based on the community field study, rating scale and other devices of social science. We are rapidly accumulating a voluminous, though perhaps somewhat superficial, body of literature on social status and its significant relationships to personalities and social institutions.

Deserving of a major portion of the credit for the somewhat dubious achievements to date is the anthropologist and sociologist, W. Lloyd Warner, who is senior author of one of the four books to be discussed here and whose concepts and methods are more or less directly reflected in the other three volumes. Eight years have passed since Warner and Lunt published *The Social Life of a Modern Community*, in which they presented the status levels of a New England town as the prototype of the American class structure. Their work, though interesting and valuable, has been characterized by many as somewhat naive in conception and unfortunately shoddy in methodological rigor. A basic fault repeatedly cited in commentary on this and succeeding works of Warner and his many collaborators is the failure of the writers to make explicit and clear some procedure which would make it possible for other social scientists to classify the members of a community in the style practiced by the Warner school. It has been claimed that the criteria of status are too variable, unstandardized and unquantified to be usable by an outsider.

[1] Warner, W. L., Meeker, Marcia, and Eells, K., *Social class in America*. Chicago: Science Research Associates, 1949.

Hollingshead, A. B., *Elmtown's youth*. New York: John Wiley, 1949.

Havighurst, R. J., Taba, Hilda, *et al.*, *Adolescent character and personality*. New York: John Wiley, 1949.

Stendler, Celia Burns, *Children of Brasstown*. Urbana: Univ. of Illinois Press, 1949.

The Measurement of Social Status

It is perhaps in a belated attempt to shore up this sagging fundament in a somewhat pretentious edifice that Warner and his two collaborators now give us *Social Class in America.* Save for its first chapter, which delineates the Warner conception of social classes, the book is wholly concerned with the development and use of two techniques for the measurement of social position called Evaluated Participation (E.P.) and the Index of Status Characteristics (I.S.C.).

Of the two, the more basic is the Method of Evaluated Participation, for it is by means of it that communities are originally stratified by the social analyst, and the various individuals in the community allocated to one or another of the social-status levels so derived. In employing it, the status analyst uses one or more of six separate but related devices. They are all evaluative and judgmental, i.e., rating techniques, complementary to one another, but use of one or more of them may suffice to place a given individual more or less accurately in the status scheme. Before using any of them it is first necessary for the investigator to derive what is called a "Social-class Configuration" or "over-all status schema" for the whole community from intensive and prolonged interviews with articulate and status-conscious persons in the community. Such interviews with informants obviously require the best of rapport, and Warner, Meeker and Eells indicate that several such informants are necessary, though they do not reveal what might be acceptable as a numerically adequate sample, saying merely that numerous persons of various social levels are desirable. "A good judge ordinarily is someone who has been in the community all of his life, who thinks about the people of his town in status terms, and who has a vocation that relates him to all social levels."

Granting that this is methodologically adequate for the derivation of an over-all status schema for a community—a point about which more comment later—one is now prepared to place the individuals in the sample in their respective status levels by the various rating devices of E.P. These are:

1. *Rating by matched agreements* of several informants on the placement of many people in the several classes. This is ordinarily accomplished by using the same data employed to construct a composite "Social-class Configuration," since it includes and is largely built upon lists of names of individuals volunteered by informants in their descriptions of the status levels of the community. Apparently, when many informants agree in assigning a given individual to a particular status level, the analyst allocates him accordingly. The degree of agreement required for such placement, unfortunately, remains unstated by the writers, and they appear in discussing the technique of matched agreements to be far more concerned with establishing by it the reality of status levels and status consciousness in the thinking of their informants than in instructing us in its use.

2. *Rating by symbolic placement.* Here an individual is rated by the analyst

as being in a particular social-status level because he is identified by informants with various symbols of superiority or inferiority such as region of residence, social traits and membership character.

3. *Rating by status reputation.* An individual or his family is assigned to a given status level by the analyst because informants say he has a reputation for engaging in activities and possessing certain traits which are considered to be superior or inferior.

4. *Rating by comparison.* In this case the subject or his family is rated as being in a particular status category because informants report him to be equal, superior, or inferior to others whose status level has been previously determined.

5. *Rating by simple assignment.* A person is placed in a given status category because one or more "qualified" informants so assign him.

6. *Rating by institutional membership.* In such case an individual is assigned to a particular status by the analyst because informants report him to be a member of certain institutions which are ranked as superior or inferior. The institutions referred to are families, cliques, associations and churches.

The social-status levels or classes and their membership content derived from these several rating methods constitute for Warner, Meeker and Eells the ultimate class structure of a community. Its validity rests, they say, in effect, in the thinking of their informants themselves—"they are the final authorities about the realities of American social class."

The Index of Status Characteristics is a shorter, more convenient method of measuring social status, offered in the realization that the method of Evaluated Participation is complicated, laborious and time-consuming—and, of course, very expensive. The I.S.C. is computed for each individual by rating him on a seven-point scale for each of four status attributes, namely Occupation, Source of Income, House Type and Dwelling Area, and then multiplying his several ratings by appropriate weightings derived by a regression equation to get a weighted total. The I.S.C. score of an individual is found to predict his social-status level (as determined by the method of Evaluated Participation) in 84 per cent of the cases—for "old Americans." For "ethnics" the percentage of correct placement is smaller, being 72 per cent of the cases.

Warner, Meeker and Eells have on the whole done an excellent job of both carrying through and reporting on what might appear to many as a tedious project. Though apparently the major goal of their work is measuring the social status of people *per se*, it will undoubtedly prove useful in studying a wide range of behavior related to status and class, and no student of these phenomena can affort to neglect it. However, a major flaw, threatening to invalidate the entire superstructure, lies in the inadequacy of the sampling of informants whose opinions are used to construct the over-all social-status schema. The writers point out almost piously that the realities of social class exist in the final analysis in the thinking of people themselves, *which means that social-status*

levels exist as public values, i.e., are a public-opinion phenomenon. Such being the case, it would seem incumbent upon scientists concerned with accuracy and thoroughness to content themselves with not less than a representative cross-section of informants in building up the basic status schema upon which all else in their system depends. This, however, is not done, and their informants, instead, are a highly selected, highly articulate, higly status-conscious few who appear to be disproportionately representative of our higher social, economic and educational strata. The status levels constructed and the membership in them determined by the methods of Evaluated Participation are thus necessarily dictated by this unrepresentative group. The I.S.C. predicts, also, no more than what would be this group's judgment of an individual's social status. Another serious limitation of the study, of course, lies in its being based upon a single small community, so that the I.S.C. scores presented have meaning only in terms of it.

Coming of Age in the Status System

In August B. Hollingshead's *Elmtown's Youth*, the measurement of status functions primarily as a means rather than as an end in itself. Hollingshead has in this work stratified his population into status levels and then gone on to show in what ways and to what extent the status level of the parent conditions the life of his offspring, particularly in the high-school years. To those who are familiar with the Lynds' Middletown books, the Warner group's series of community studies, and others of similar mold, the story told will not be very new, for there is perhaps overmuch in this volume that has been said several times before. Nevertheless, the beastiliness of the American pecking order is sufficiently fascinating to make *Elmtowns' Youth* rewarding reading. Hollingshead has so faithfully reproduced the jungle that we all grew up in and live in that one meets himself or a friend or an enemy on almost any page.

The book is offered as an analysis of the way in which the social system of a Middle Western Corn Belt community organizes and controls the behavior of high-school-aged adolescents reared in it. This analysis is achived by describing the relationships existing between the behavior patterns of 735 such subjects and the positions occupied by their parents in the community's class structure. The seven arenas of social interaction examined are those of the school, the job, the church recreation, cliques, dates, and sex life.

In stratifying the families of his sample, Hollingshead adopted a somewhat roundabout method. He first derived a "control" or reference list of 20 families alleged to belong to one or another of the five status levels which were believed to exist in the community. Then he had a carefully selected group of raters indicate the status levels of the 535 families in the sample by equating their statuses with that of families in the control list. Although it is asserted that "care was taken to select

persons from every ethnic and religious group and from every stratum, so that raters would be representative of all sections of the population," it is difficult to believe that the 31 individuals selected were actually adequate for this purpose. Seldom, surely, have so many represented by so few. Probably a more accurate picture of the raters is the following: "Some were in key positions in the institutional structure, others performed functions which brought them into contact with many types of people; and a few were 'plain' citizens. All knew the community through years of living in it, and they were willing to discuss many aspects of community affairs." To this reviewer it is impossible to see this group as "representative of all sections of the population."

Hollingshead is at his best in the sections dealing with the life patterns of the youth of the several status levels, and in describing how the desire for participation, status, self-expression and self-realization in general of working-class youth is systematically frustrated by the power and influence of the ruling class in Elmtown. Often the impact of the data is enhanced by letting the kids tell it in their own words, for they convey, far more poignantly than any objective-minded social scientist allows himself to, the bitterness and unhappiness our social order engenders.

Although *Elmtown's Youth* is especially recommended to American parents by its author, it is unlikely that it will be widely read outside of academic circles, and will more probably be found useful as supplementary reading in university courses in social anthropology and social psychology. If so used, however, it might be well for the instructor to point out that *it is not some disembodied class system that pushes people around in America's Elmtowns, but a flesh-and-blood ruling class and the power it wields.*

Character Reputation and the Status Order

Of the studies under review, probably the most mature, and certainly the most elaborate, is *Adolescent Character and Personality* by Robert J. Havighurst and Hilda Taba, in collaboration with 15 other scientists of The Committee on Human Development of The University of Chicago. The book is a companion piece to *Elmtown's Youth*, being based upon research in the same community and in part on the same boys and girls. But whereas Hollingshead's study was concerned with the behavior of several age groups of adolescents in various spheres of social interaction, this work is an intensive analysis of character in sixteen-year-olds alone. Though concern with the status order is prominently featured in the report, the study is not entirely centered upon status as a character determinant or correlate. Instead, "social-class position" is considered as only one of eight major factors in character formation, the others being school achievement, intelligence, values and ideals, self-adjustment, social adjustment, moral beliefs, and religious activity.

Recognizing that character is a word with many meanings, the au-

thors indicate what they mean by the term more precisely by indentifying it with the phrase "moral character," and further, as "that part of personality which is most subject to social approval." It is added, not too clarifyingly, that having a sense of humor (which is certainly socially approved) is not a part of character but part of that residual something we label "personality."

After conceiving of character as a composite of moral traits, namely, *honesty, responsibility, loyalty, moral courage* and *friendliness*, the next problem, of course, was that of devising some measure of it. Largely on the basis of the monumental studies of Hartshorne and May two decades ago, and the accumulated experience since, behavior was rejected as a usable criterion, and the individual's character reputation was adopted. Reputational ratings for each of the 5 traits as the measure of character were then secured from the individual's teachers, age mates, Sunday-school teacher, school principal, scout leader, and employer. Such judgments were obtained by means of four specially designed instruments, each differing in form, detail and content, but similar in that they described the same general type of behavior manifestation for each trait. Two of them were used with the adult group in the community (teachers, scout leader, etc.) and two with the age mates of the subjects, the object being that of checking on the stability or consistency of the judges' opinions. The mean of the individual's scores on each of the four measures was regarded as the ultimate criterion of his character reputation. Considering the difficulties of measuring such a thing as character, the "reliability" coefficients obtained for the four scales might be considered at least fair at worst, and at best quite good. The lowest was .46 while the highest was a substantial .86. The validity of the instruments is not established.

Adolescent Character and Personality is a rich and meaty offering which contains many challenging leads and insights. But even its authors practically acknowledge that character reputation as they have measured it is, at most, merely an index to the degree to which adolescents conform to the standards of middle-class conduct. They find character reputation to be correlated so slightly with religious activity, moral beliefs, values and ideals, self-adjustment, social adjustment, and even with intelligence and social-class position, that the degree of relationship is of little predictive value. The highest of these correlation coefficients is .52 between social-class position and character reputation, but this is distinctly lower than the correlation of character reputation and school achievement which is .74. Since school achievement, like character reputation, is in a sense a measure of conformity to middle-class standards, it is not surprising that there should be a substantial degree of relationship between it and another measure of conformity.

Although this book makes a contribution to our knowledge of character, its worth as a piece of scientific writing is seriously impaired by

an apparently unquestioning adoption and tacit, if not entirely explicit, approval of middle-class patterns of character ideals as the goal toward which character education should strive if the good life in America is to be achieved. It is disturbing to find social scientists so class-centered in their thinking, especially when they themselves recognize that the majority of people are not middle class. Most, moreover, might not want to be.

Children's Awareness of Status and Class Distinctions

Celia Burns Stendler's *Children of Brasstown* is by far the briefest of the research reports considered here, being a monograph of slightly more than 100 pages. But it is by no means the least worthy or interesting, since it is a pioneer thrust into largely unexplored territory. It is a study of "awareness of the symbols of social class" on the part of the children of Brasstown, a New England community of about 15,000 people.

After a brief and quite inadequate "examination" of three other conceptions of social classes, Miss Stendler adopts the Warner definition of class "as a group of people ranked by members of the community in socially superior and inferior positions according to certain socio-economic factors." The social-class position of each of the 107 school children from the first, fourth, sixth and eighth grades who served as subjects was determined by the rating assigned to his parents by a group of five judges. These consisted of three professional men, one businessman and one housewife, who were selected because of their familiarity with the families and for other reasons.

Since Stendler's study is primarily concerned with children's awareness of the *symbols* of social class in the sense of that term previously indicated, one might have expected the author to define the phrase, "symbol of social class." It is disconcerting to find no such definition. Instead, the reader is left to shift pretty much for himself. From the few examples given and the items identified as symbols in the test instruments employed, Miss Stendler seems to mean by a symbol of social class any social attribute or behavioral manifestation that people might use as a criterion of class distinction. Her study involves only a limited number of such characteristics.

The author's procedures in ascertaining the class awareness of her subjects involve a combination of interview and questionnaire techniques in which the approach to consciousness of status and status symbols is often so devious as to yield results of quite dubious significance. The interpretations and conclusions are highly subjective in the main, and at times seem almost gratuitous. First, an interview was held in which children were questioned about their out-of-school activities and their choices of friends as these might indicate class biases, etc. In a second interview they were asked to rate, in economic terms, four sets of

pictures illustrating jobs, houses, clothing and recreation which adults had previously adjudged typical of one or another of the several classes. They were also asked to rate themselves and some of their classmates in economic and social terms. Class terms, such as upper-middle or working class, were not employed in such ratings, but instead the children were asked whose parents had more or less money than average, or whose families were high or low in society. This discrepancy in class terminology is unexplained, as is indeed the basis of much of Stendler's other procedure, and it seriously confuses the meaning of her results. A fifth set of pictures was employed in an attempt to find out if, along with increasing awareness of class distinctions, children were also developing associations of good and bad conduct in relation to them. The pictures portrayed children in various acts such as stealing, shooting craps, smoking, fighting, cooperating, studying, sharing toys and returning money found, and subjects were asked whether the child portrayed had little or much money or was high or low in society. A final technique was a *Guess Who* test requiring the child to guess who in his classroom best answered each of 40 brief descriptive questions such as: Whose parent doesn't have a very good job? Who never has any spending money? Who has the nicest home? The object here was to see what relationship, if any, existed between the item in the test and the social-status level of the child mentioned for that item.

In summing up the fruits of her observations (and insights), Stendler characterizes the development of class and status consciousness as progressing through four stages.

> Stage I is that of *Pre-awareness,* and is the condition found to exist for most children below the fourth-grade level, when such terms as 'rich' and 'poor' have little real significance for them.
>
> Stage II, the *Beginnings of Awareness,* extends from the fourth grade to beyond the sixth, and is characterized by signs of increasing class consciousness. The child is apparently more conscious of symbols of lower classness than others, can pick out children with "not much money" in his schoolroom, and can judge them on the basis of their clothes and what they bring to school. He is largely unaware of housing and occupation as class distinctions.
>
> Stage III is that of *Acceptance of Adult Stereotypes.* It begins before grade 6 and continues through grade 8. As the name implies, this is a state of cognitive organization in which the child closely approximates the adult patterns of beliefs and attitudes concerning social classes, including the many contradictions about class which are prevalent in our culture.
>
> Stage IV, *Recognition of Individual Differences,* was achieved by only a few children in the study, and represents a condition of mind wherein the child recognizes the meaning social-status symbols have for most people, but judges people not merely in terms of their class-membership character but in terms of their individual merits as well.

Miss Stendler's delineation of these stages, in the face of her indirectness of method, is quite bold, and of necessity must be accepted

with considerable caution until verified in further research. She has, nevertheless, made an important beginning in an area which has hitherto had no systematic exploration.

Summary Comment

Though there is obviously in the content of these studies a common bond that ties each of the four into relationship with the concepts of social status and class, each is so discrete in its aim and contribution that an over-all evaluation is rendered difficult for lack of a common problem as a reference point. Each has been of necessity discussed and evaluated in terms of its specific content and achievement, since no central theory or systematic body of principles exists which might define a "Science of Social Classes". The work in this field to date has achieved little in the way of systematization, but consists in the main of exploratory efforts whose results scarcely transcend the descriptive level of discourse. The facts garnered are numerous, but they are not easy to unify into a single theoretical structure. We have anthropological facts, sociological facts, psychological facts, or at least facts gathered by workers in each of these disciplines. And the varied disciplinary affiliation of the facts constitutes in itself something of a barrier to the achievement of an integrated conceptual formulation. We do not yet know whether the systematization of the data we are accumulating will lead to a psychology or to an anthropology or to a sociology of social status and class. The four studies we have been considering make contributions to all three, but their contribution mainly enlarges our store of descriptive material without appreciably contributing towards theoretical advancement in any one of these fields.

A particular conceptual difficulty in most of this research is the lack of adequate differentiation among such concepts as stratification, status and class. These terms are generally used interchangeably and with a breadth of reference that renders them confusingly vague. There has been, however, very noteworthy methodological advance, and with further refinement this may possibly lead to the eventual satisfactory resolution of the conceptual predicament. In Warner's *Evaluated Participation* and his *Index of Status Characteristics*, we are at least moving toward more precise operational definitions of these terms. The Index of Status Characteristics, moreover, holds considerable promise for the facilitation of further research of a somewhat more quantitative and systematic character, since the levels of social status derived from such an index constitute a reasonably objective and meaningful datum with which to work.

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BOOK REVIEWS

POSTMAN, L., & EGAN, J. P. *Experimental psychology.* New York: Harper, 1949. Pp. vii+520. $4.50.

The announced goal of this text is "to give a survey of the main empirical findings and functional relationships in selected areas of experimental psychology, with special emphasis on the control, manipulation, and measurement of variables" (p. xiii). Recognizing the impossibility of covering the field adequately in 500 pages, the authors have limited themselves to the areas of the receptor and perceptual processes (9 of the 20 chapters and 40% of the total pages), learning and retention (6 chapters and 35% of the pages), behavior in social situations (38 pages), reaction time and association (36 pages), analysis of judgment (22 pages), and emotional behavior (18 pages). At the end of every chapter are one or two experiments (30 in all) suitable for the typical laboratory course, designed to illustrate some of the methodological problems described in the chapter. The page or two of accompanying references are arranged in order of the sub-headings of the chapter.

The question naturally arises, Is this an acceptable guide for *the* course called "experimental"? The answer depends first upon one's conception of the pattern of objectives in the undergraduate program. It may be argued that the paramount aim, with either the student of the "liberal arts" or the pre-doctoral candidate, is to impart a realistic and working comprehension of the scientific method, particularly as it is applicable to behavior. Operationally, what does (or, at least, should) a psychologist do when he investigates? What are the premises, the potentialities, the pitfalls, the inherent limitations of his procedure? These insights are not acquired easily, even at the verbal level; it is doubtful if much automatic transfer can be expected from the first chapter of an elementary text or from a detailed description of retinal processes. Perhaps this objective will best be attained by guiding the student through a few representative problems, constantly referring on the one hand to the ideal paradigm of experimental procedure and on the other to some outstanding examples of its application. To this end the second-year course in psychology, particularly, may be dedicated. The survey of general principles can be assumed to be the preserve of the introductory course, and the elaboration of special techniques and the minutiae of findings can be relegated to the more advanced courses. But the backbone of the whole program is this course in Experimental Psychology—a thorough indoctrination into the basics of experiment and a first-hand introduction to the experimental literature of psychology.

If this personal prejudice is accepted, the conclusion will probably emerge that Postman and Egan have not provided the ultimate text-

book for such a course. While much research is digested and passed on in the form of somewhat idealized generalizations, too much responsibility is left to the instructor for that most vital task, the critical analysis of the rationale and design of experiments. At the same time, the desire to relieve the student of the "burden" of encyclopedic documentation has been carried too far: there are virtually no references to original sources within the body of the chapters, and the grouped citations are not keyed to the discussion. The result is a highly effective barrier to the student who might otherwise have been tempted occasionally to pursue a point beyond secondary accounts. So our attempt to reach the twin goals of the course receives less than maximal help from the book.

Of more specific evaluation, there will inevitably be much on both sides. Some will disagree with the selection and relative weighting of the major topics. Others will say that considering the great preoccupation with the receptor processes, too little space is devoted to their electrical investigation; receptor theories other than the place and the Young-Helmholtz are worthy of discussion; Helmholtz merits at least one non-hyphenated mention; animal studies of taste preference should be given some space. There will be criticism of a few definitions that somehow fail to come off, and of some statements that might be interpreted to be either descriptions of actual experiments, or logical analyses of problems, or hypothetical formulations, or firmly grounded explanations. Regrets will be expressed over the slightly inconvenient form and location of some tables and figures, and such clerical errors as citing a chapter of Dashiell's in the *Handbook of Experimental Psychology*. But few will question the general workmanship of the book, and many will welcome the straightforward style as a relief from both the closely-written compendium for advanced students and the palsy-walsy primer for beginners.

The final judgment of the book must obviously be a joint function of the book itself and of the purpose for which it is being considered. It will probably find wide use as a combined textual discussion and laboratory manual in experimental courses which are somewhat limited with regard to facilities (laboratory or library), prerequisites, or objectives. In the program discussed above, its most logical adoption would be as a supplementary text for the first-year course, particularly if laboratory work is to be provided.

Frank W. Finger.

University of Virginia.

Dennis, W. *Readings in general psychology*. New York: Prentice-Hall, 1949. Pp. xi+525. $3.75.

Our inflated enrollments in the introductory course have forced us to depend almost entirely upon pre-digested psychology as found in texts and digests of research. Now students in large numbers will have

ready access to 71 original reports by authors ranging from Aristotle and Leonardo da Vinci to Neal Miller and Floyd Ruch. Many of the selections are abridgments, but not digests. In size they range from one to a dozen or more pages. The selections are grouped into eleven chapters with conventional titles. Within chapters they are sometimes arranged in logical and sometimes in historical sequence. Each selection is preceded by a brief note which places it in its proper topical setting. Quite often there is a little information about the author and the significance of his contribution.

In approaching the evaluation of an anthology of this kind one is led to wonder how broadly it covers the field and whether its selections are representative of psychological writing and research.

The field covered is broad, the chapter headings being as follows: sight, hearing and other senses, perception, emotion, motivation, learning and retention, reasoning and thinking, intelligence, social behavior, personality, and psychological development. One may question whether sensory and perceptual processes should be given more attention than learning (110 as against 47 pages), but the other fields seem equitably represented.

The teacher who expects to find a book of selections from current research will be somewhat disappointed. Eighteen of the 71 selections were written before 1900. Of the remaining selections, more than one half originated before 1930. Only eight were written within the last decade. Perhaps one reason for such a preponderance of classical and pioneer studies is the fact that so much of recent writing is too technical for ready digestion by beginners. Rather generally the selections appear well-chosen and they should appeal to students as well as to instructors who have not already read them.

Norman L. Munn.

Bowdoin College.

Valentine, Willard L., & Wickens, Delos D. *Experimental foundations of general psychology.* (3rd Ed.) New York: Rinehart, 1949. Pp. xxi+472. $3.00.

The readability of psychology textbooks continues to improve. Valentine was a clear and effective writer, and his first chapter, "Magic and Science" is an excellent initial step for the beginning psychology student. Wickens completed the revision of this well-established text by adding and deleting experiments from almost every chapter; completely revising the chapters on Perception, Conditioning, Learning; and adding a new last chapter, Personality. He purposefully omitted a chapter on Social Behavior, a decision, however, with which we do not concur.

The number of sectional and paragraph headings has increased, showing more distinctly the method, results, and summary of the re-

ported experiments. This third revision shows the same trend as the second over the first: more complete integration between the different experiments resulting in a more coherent pattern of subject-matter treatment within each chapter. In contrast to the original edition, the book now closely approximates the typical introductory text with, of course, an emphasis on the experimental findings.

A "revision" usually attempts to improve the presentation within the framework of the original publication. The postwar improvements in textbook printing are seen in the better grade of paper, sharper printing, and more distinct figures and tables. Our chief impression still remains the clarity and interesting style with which the experimental findings are presented and linked together. This effectiveness in presentation was characteristic of Valentine, but the co-author's own contribution is significant. To maintain the contemporary emphasis, the refinements in style, the new content and added comments were introduced without noticeably changing the established pattern of the book. Instructors familiar with two precursors of this new edition will find a better volume, more effectively accomplishing its purpose.

The contrast in content and emphasis between this volume and the introductory text recently written by Guthrie and Edwards, *Psychology: A First Course in Human Behavior*, demonstrates the futility of passing judgment as to which is the "best buy" for use in teaching psychology. Those instructors who prefer the experimental and factual approach are likely to endorse the book under review. If one's teaching of the first course is biased toward principles and applications, this book could best be used only as a reference. This dual purpose as either a text or as a reference book is at the same time its strength and weakness. Many teachers may prefer other introductory texts that attempt to carry the entire load of introducing the student to psychology; or, as references, we may choose other and more specific source materials for showing the experimentalist at work and the factual subject-matter of scientific psychology, providing our own integration and interpretive comment.

While this reviewer does not prefer an experimental and content-laden first course, we recognize and respect the many instructors who do. These individuals will appreciate and profit from the extension and improvements appearing in this latest revision.

STANFORD C. ERICKSEN.

Vanderbilt University.

MURPHY, GARDNER. *Historical introduction to modern psychology.* (Rev. Ed.) New York: Harcourt, Brace, 1949. Pp. xiv+466.

Although it is the last third of this book, outlining the contemporary scene, that differs most from its 1928 predecessor, the minor changes in the earlier parts supply interesting clues to the influence of two active

decades on the author's organization and interpretation of the historical trends leading up to the present.

The organization is now quite simple. There are four parts, The Antecedents of Modern Psychology, The Rise of the Research Spirit, Contemporary Psychological Systems, and Some Representative Research Areas—each composed of several chapters. The chapters in turn are broken up into several sections, a device that may disturb the continuity, but helps the reader grasp the writer's plan.

The first edition began with a few paragraphs on the Renaissance. The present one prefixes a brief but illuminating summary of the psychology of a few of the Greek philosophers and even some speculations about primitive psychology.

The thirteen chapters of Part Two that describe the rise of the research spirit have been only slightly altered. British associationism, its origins, variations, and fate, are still treated rather fully. There is the same sympathetic insight into the strange personality and convictions of Fechner, "reaching in one direction and another . . . . bewildered by the complexity of the spiritual heritage," struggling desperately to perceive unity in this life and world. The impact of evolutionism is strongly emphasized. More space has been allotted to William James as new material has come to light. His spirit refuses to die even though, as Murphy puts it, "the research tools which he distrusted have become the chief keys to a technical world for which he had no taste."

History, including psychological history, is often written as if it were the product of a succession of brilliant and vigorous idea men; Murphy, however, when introducing a topic, sketches the philosophical and scientific background, portraying the political and economic culture in which the ideas will grow or die. A quotation will illustrate his plan and his manner. "While behaviorism is expressly a psychological movement, it is an expression of a much more general movement in science (and in the philosophy of science). When it first appeared, it seemed a lonely island; but like all such islands, physical and intellectual, it proved to have many relatives, both visible and hiding."

In Part Three, behaviorism, Gestalt, field theory, and psychoanalysis come up for discussion, in essence at least. The conditioned response is included in the chapter on behaviorism. Other theories of learning and the role of motivation in learning are treated in a separate chapter. It is easy to imagine some learning theorists objecting to the organization of learning theories around associationism and to calling the chapter Modern Conceptions of Association. The chapter on Freud amplifies his later work on ego psychology. The various new movements in psychoanalysis—and their splinters—have a chapter to themselves with the title The Response to Freud. Each system receives a sympathetic hearing, with only occasional criticism.

Part Four attempts to bring certain areas of research up to date:

intelligence, physiological psychology, child psychology, social psychology, and personality. It ends with a few pages of general interpretation. This is the part which will receive the most criticism. The author anticipates this by noting that the selection of topics is a personal one, that he has omitted comparative psychology, several well developed areas of applied psychology, abnormal psychology, and others. He regrets that he has not digested the military research—even in social psychology—carried out during World War II. Although Murphy has often stressed the systematic importance of motivation and the first edition had a chapter on instinct, this topic now has no chapter, nor section of a chapter, to itself.

No one, to be sure, could balance a book like this one so as to bring all readers' interests into equilibrium. Most psychologists will find almost everything they have a right to expect treated here, and treated with respect as well as depth. Graduate students and college seniors—if they are not dismayed by occasional sixty-word sentences—will find clarity as well as breadth.

DONALD M. JOHNSON.

Michigan State College.

HUMPHREY, G. *Directed thinking.* New York: Dodd, Mead, 1948. Pp. 229. $3.50.

HUTCHINSON, E. D. *How to think creatively.* Nashville: Abingdon-Cokesbury Press, 1949. Pp. 237. $2.75.

Gradually we psychologists learn more of the social responsibility entailed in our roles as scientists. One way of fulfilling this obligation, and at the same time preventing the prostitution of our science by the quack, is in writing the results of our researches so that they may be understood by our fellow citizens. Both Humphrey and Hutchinson have performed this mission in their books about the reasoning and creative thinking process.

From the point of view of his fellow scientist, Humphrey's work is a paragon in popularization. He cites much of our experimental literature as proof for his assertions; he constantly reminds his reader of the fundamental tenets of our scientific approach. For instance, in using the results of experiments by Ach, Maier, Thorndike, and others, he skillfully conveys the relevance of these researches to his reader by interweaving them with concrete, practical examples from everyday experience. In discussing the erroneous nature of the trial-and-error theory, Humphrey manages to help the reader understand that such a theoretical mistake in itself may be helpful, "because it has brought the facts into the open." Humphrey broadly conceives the process of "directed thought" or reasoning (as contrasted with "free thought" or reverie) as including such aspects of thinking as imagery, insight, concept formation, sym-

bolism, and generalization. The wide coverage at times induces in the reader an uncomfortable sense of hodge-podge. This lack of integration is assuredly more the result of the state of psychology than of Humphrey's skill as narrator. The psychologist, as scientist, however, will look in vain for new interrelations, which might have appeared in the course of Humphrey's evaluation of literature in this area.

Hutchinson's work will do little to help laymen gain appreciation for the use of more modern, experimental, scientific research techniques in such difficult areas as creative thinking. Although he stresses the importance of the emotional and unconscious factors which enter the process of thinking, his own methodology is infected by a late 19th century introspectionism. He bases his account upon the results of a questionnaire on creative effort given to some 250 notable American and British thinkers, (e.g., Aldous Huxley, Arnold Bennett, Bertrand Russell), rich anecdotal reports he has collected from magazines and books, and his own conversations and thinking with engineers and inventors. Hutchinson neglects the valuable supports he might have gained from the scientists whose techniques allow experimentation with those unconscious factors which are barred to introspective techniques. Toward the end of his work, his weak faith in the scientific method is revealed in such an assertion as, "Creative thought . . . is the history of these factors in multiple and ever-changing patterns. For this reason no one will ever write the whole account, for it is written not in words but in unfathomable experience" (p. 196).

Throughout Hutchinson's book one senses he is not writing merely for the layman. Starting this work when he was studying at Cambridge with McDougall and Bartlett in the early twenties, Hutchinson makes a masterful attempt to integrate his considerations about the creative process, both in its logical and intuitive facets. His result is far superior to Wallas' stages of creative thinking. Yet, it does not result in a theory readily subject to experimental testing. Only part of Hutchinson's success in unifying his treatment, as contrasted with Humphrey's, comes from his exclusion of findings in such tangential areas as concept formation; the other part of his success is due to his far greater willingness to make assertions without supporting experimental facts.

There are important differences between the two books in the extent to which they are permeated by the scientific as contrasted with the philosophic approach. Both writers have done a very real service to the lay reader in conveying the impact which the more recent emotional-motivation emphases in psychology have had upon treatment of the "higher" mental processes. Both books stress the non-logical, unconscious nature of the psychological process even in such a stronghold of pre-Freudian psychology as thinking.

HAROLD GUETZKOW.

University of Michigan.

SWENSON, ESTHER J., ANDERSON, G. LESTER, & STACEY, CHALMERS. *Learning theory in school situations.* Minneapolis: Univ. of Minnesota Press, 1949. Pp. 103.

This monograph should be of interest to the practical-minded educator as well as to the student of learning theory. It includes abstracts of three doctoral dissertations on the psychology of learning in the schoolroom situation, and an introduction which attempts to extract from these studies certain theoretical implications.

The findings, many of which have practical significance for instructional purposes, are too numerous to be recounted in this brief review. Dr. Swenson, working with second grade pupils, reports a highly significant superiority of a "meaning" or "generalization" method of teaching simple facts of addition as compared to two "drill" methods. This superiority was especially evident when a transfer criterion of learning was used.

Dr. Anderson applied similar type methods in teaching fourth grade students arithmetic skills. When the criterion of learning was response reproduction—i.e., the pupils had to recall specific knowledge they learned—". . . there was little difference between the pupils who had relied on repetition of specific elements and those who had followed procedures which encouraged understanding and meaningful organization. But when the criterion was the ability to apply what presumably had been learned to new situations . . . the pupils who had been encouraged to comprehend mathematically what they were learning and to capitalize meaningful relations were significantly superior." This, however, was not true for all the students; pupils of relatively low ability and good achievement learned better under the drill method while students of relatively high ability and poor achievement learned better under the meaning method.

Dr. Stacey tested sixth grade children in their ability to solve certain verbal problems (select the word that did not fit in a group of five words). Five methods of training were used, each varying in the degree of information given to the subject. The results were that "active participation involving self discovery" was superior to "passive participation involving recognition . . . of information previously provided. . . ." The results of this study of problem solution tended to support "Thorndike's conclusions concerning the efficacy of reward and the inadequacy of punishment"—especially when the self-discovery technique was used.

The theoretical implications of these three experiments are integrated in the introduction written by Dean T. R. McConnell of the University of Minnesota. Quoting extensively from Hilgard's *Theories of Learning* to support his own analysis, McConnell considers the initial two studies of the monograph as being a test of the "relative efficacy of learning" according to the association and field conceptions of learning.

The validity of this analysis rests upon the premise of the *existence* of *a field theory* and *an association theory* capable of generating deductions relevant to the experimental situations of Drs. Swenson and Anderson. This reviewer does not think this to be the case. At best, the terms "field" and "association" have denotative meaning only if they refer to formulations which certain writers for various reasons group together. The connotative meaning of these terms, which vary markedly from individual to individual, should not be confused with scientific theorizing. In this monograph, association theory is equated with some statements from Thorndike's unsystematic writings. The evidence creates serious doubt about the wisdom contained in such statements. This, however, does not justify the generalizations made to association theories as a group. Variations in motivation and reward which play such an important role in some of the association theories are almost completely ignored in the experimental aspects resulting from the "meaning" type of pedagogy. Nor is any cognizance expressed of Hull's statement about the practical educational implications of his associationistic theory: "On the basis of the preceding principles it seems reasonable to suppose that if children from an early age were systematically trained to find the solution of genuine individual problems by means of their own symbolic processes, intellectual education might be far more effective than it is at the present."[1]

The lack of rigor of the theoretical interpretation should not detract from the well-executed, "tough-minded" research contained in this monograph. It merely emphasizes the unfortunate hiatus which exists between practical minded educators and learning theorists.

HOWARD H. KENDLER.

New York University.

MEAD, MARGARET, *Male and female: A study of the sexes in a changing world.* New York: Morrow, 1949. Pp. xii+477. $5.00.

In this new volume, Margaret Mead gives us the benefit of a quarter of a century's observations on male and female in a variety of cultures. Her purpose in writing the book is "to give to the reader the positive findings from a comparative study of culture about similarities, about the essentials in maleness and femaleness with which every society must reckon, and regularities as well as differences" (p. 32). Her emphasis throughout is biosocial and provides a corrective to her previous apparent overplaying of cultural at the expense of biological factors.

By way of introduction Dr. Mead takes the reader on a verbal field trip and by a succession of concrete word pictures permits him to share at "first-hand" some of her own experiences in the South Seas. The

[1] Hull, C. L. *Conditioning: outline of a systematic theory of learning in the psychology of learning.* 41st Yearbook, Part II, National Society for the Study of Education. Chicago: Univ. of Chicago Press, 1942.

three remaining sections constitute the main body of the book. Part II, "The ways of the body" is a child's-eye view of the varying ways in which he discovers and assumes his sex role in the seven primitive societies that the author has intensively studied. Part III, "The problems of society," is a reciprocal of Part II, revealing the varying ways in which these societies have patterned the sex roles of their people. Finally Part IV brings the author's ethnological insights to bear on "The two sexes in contemporary America." Following the presentation of the central material there are four supplementary sections which elaborate the points in the text and provide sufficient documentation to compensate for its meagerness in the author's earlier popular works.

Whatever the reader's bias, he can hardly fail to recognize *Male and Female* as a landmark in a chaotic field, providing a new frame in which to view old problems. The unique feature is the handling of each problem by means of direct intercultural comparisons,—a treatment conducive to objectivity. According to Dr. Mead, an important lesson preliterate peoples can teach us is that underlying wide diversity in superstructure, all cultures share certain basic regularities in biosocial development which differentiate male and female the world over. As a result, the author sees security for the male to depend on achievement; that for the female, on nurturing. The moral is for the good society to take account of these fundamentals in assigning roles to its men and women, while at the same time permitting sufficient freedom of choice to allow full expression of the abilities of both sexes. Having taken this stand we are surprised to find the author in sudden retreat, in an attempt to escape from what she fears may be too much freedom for women, threatening both their men and their femininity. It would be more in character for Dr. Mead to be seeking ways of channeling the freedom she fears into new, more cooperative patterns of behavior between the sexes that would remove the sources of threat to both.

Although the viewpoint expressed in *Male and Female* makes interesting reading, the scientist will be chiefly concerned with the validity of its basic assumptions. Some of these we find rooted in the hypothetical psychodynamics of the preverbal infant. "So the female child's earliest experience is one of closeness to her own nature. The little boy, however, learns that he must begin to differentiate himself from this person closest to him" (p. 148). In view of the aheuristic nature of such "earliest experiences," it would seem extremely risky to use them as the cornerstone of the New Society. There are other questionable assumptions which, though testable, have not yet been put to test. An example concerns women's nurturing functions, "In the case of women, it is only necessary that they be permitted . . . to fulfil their biological role, to attain this sense of irreversible achievement" (p. 160). Finally, there are those assumptions that have neglected the "negative instances" for which the author herself specifically alerts the scientist.

Thus we find that the keynote of the chapter on "Rhythm of work and play" is the notion that women are subject to periodic fluctuations in capacity, extensive research literature to the contrary notwithstanding! Throughout the volume, the reviewer had the uncomfortable feeling that evidence was sacrificed to theory, and often wished Dr. Mead had depended less on "the eye as a scientific instrument" and had actually gotten down to the counting of neckties and other things she felt so free to dispense with. So much reliance on intuition leaves the reader wondering whether the picture portrayed in *Male and Female* may not be a little "artistically out of focus."

Although a brief review cannot touch on the many stimulating ideas advanced in Dr. Mead's present work, her treatment of "The ethics of insight-giving" which she tucks away in an appendix is too important to ignore. Here she defends her conspicuous omission of social pathology on the ground that "Society, the patient" might misuse the insights and become the worse for them, as she fears may have resulted from the dissemination of the Kinsey report. It seems to the reviewer that this position involves not only the question of the ethics of insight-giving but also the question of the ethics of censorship in a democracy. Unless we accept a dictatorship of experts, society will have to be therapist as well as patient and will need all the insights it can get to cure itself. The cultism implied in Dr. Mead's arguments for witholding information seems wholly irreconcilable with her own plea for freedom through knowledge. What seems to be indicated is *more* knowledge so that *more* insights may become available, for freedom from social no less than from individual disease in a democracy must depend on an enlightened public.

From cover to cover, *Male and Female* is provocative, raising more questions than it can answer. Its chief value will be to the social philosopher but the social scientist will also find it a rich source of hypotheses. In fact, as long as the concepts presented are recognized as hypothetical rather than conclusive, this book can make a constructive contribution to our thinking in a controversial field.

GEORGENE H. SEWARD.

The University of Southern California.

LINDESMITH, A. R., & STRAUSS, A. L. *Social psychology.* New York: Dryden Press, 1949. Pp. xvi+549. $4.50.

This introductory text is offered as an exposition of the point of view that human behavior is profoundly affected by the facts that people use language and live in social groups. The book deals much more with language than with social groups, although the two are linked by showing that social organization and culture depend on language, and that socialization is mediated by language.

The first quarter of the book is a discussion of language behavior.

This section is devoted largely to pointing out ways in which language differentiates men from other animals, and with defining and describing various aspects of language behavior—symbols, symbolic environments, perception, remembering, etc. Emotional behavior is included as an aspect of language, though physiological aspects are recognized.

The second quarter of the book discusses socialization. The reader may be surprised to find that, of the five chapters offered, one deals with language development in the child and two with thinking as dependent on language and on group memberships. The other two chapters briefly discuss social roles and "the self," again with a strong emphasis on language aspects. The self is defined behavioristically as a set of regulatory responses, which results from internalizing the regulatory social responses of others.

The third quarter of this text is on personality. What unity it possesses depends on the fact that attitudes, traits, conflicts and breakdowns are related to social roles, while mechanisms such as rationalization and projection are treated as "symbolic adaptations" (Ch. 11). The chapter on deviant behavior, dealing with feeblemindedness, neuroses and psychoses, seems hard to justify in this text.

The final quarter of the book gives an elementary and rather sparse discussion of race and sex differences, and of group behavior and social change.

In this reviewer's opinion, this text fails to present an integrated account of social behavior. Most fundamental, it is largely descriptive and lacks any basic theoretical orientation. Much of the discussion is purely topical. Throughout the book, it is asserted that motives, attitudes, language, roles, etc. are learned. Yet learning theory and research are dismissed with the statement that "these materials have not proved especially illuminating or helpful for the social psychologist" (525). Freudian theory is disposed of in a section on "misconceptions of child behavior" (431). Lewinian theory is not considered. It is significant that such terms as sociometry, frame of reference, and group dynamics do not appear. Some use is made of Mead's point of view in discussing roles and "the generalized other" (175–178).

This text makes frequent use of research findings to support the discussion. However, there is no description of research methods in social psychology beyond three pages on personality tests. While the authors promise in the Preface to point out research implications growing out of their point of view, they rarely do this. The text includes sections criticizing the naïve use of such terms as mind, self, instinct, etc., reflecting the biological-behavioristic tradition.

The volume is written with marked simplicity and clarity, with pertinent and interesting quotations and illustrations. Students who have previously had an introduction to general psychology will find much repetitive material in this text. Superior undergraduates are apt to

regard this text as too elementary. Instructors who wish to introduce their students to social groups and personality organization, and to the theories and research methods being developed to measure and relate them, will probably find that this text does not meet their purposes.

GLEN HEATHERS.

Fels Research Institute.

FRANK, JEROME. *Courts on trial: Myth and reality in American justice.* Princeton: Princeton Univ. Press, 1949. Pp. xiv+441. $5.00.

This is an analysis of judicial justice and injustice in America. In fact, the subtitle would have been a better title, namely, *Myth and Reality in American Justice.*

Basically the author describes the *real* or *true* workings of our courts with which most people are not acquainted. The result is a significant study of many of the myths and legends about the law and its functioning, as contrasted with the way it really operates. The theme of "modern legal magic" recurs several times.

Judge Frank deals realistically and objectively with one of our most important social institutions. He writes, "(1) of what courts actually do, (2) of what they are supposed to do, (3) of whether they do what they're supposed to do, and (4) of whether they should do what they're supposed to do."

The result is an intriguing explanation of the uncertainty of much of our legal lore, as contrasted with the supposed certainty of the law stressed by a great many lawyers. The author analyzes in considerable detail the importance of personal factors in the functioning of the law—as it operates through human beings called lawyers, witnesses, jurymen, and judges. He shows how "court-house government" often obstructs the very justice it seeks to secure.

Judge Frank has had a distinguished career as lawyer, Chairman of the Securities and Exchange Commission, and more recently as Federal Judge; but he is perhaps better known to scholars for his own scholarly writings. All of his books and many of his law review articles have psychological themes; and in the present instance one is reminded again and again of his *Law and the Modern Mind,* published twenty years ago.

With the possible exception of the late Professor Edward S. Robinson's *Law and the Lawyers,* the best appraisals of the psychology of law have been written by lawyers, such as Leon Green's *Judge and Jury,* Huntington Cairns' *Law and the Social Sciences,* and Joseph N. Ulman's *A Judge Takes the Stand.* The present work demonstrates both in its writing and documentation the author's vast amount of reading and study in many different phases of the law, as well as in psychology sociology, anthropology, and psychoanalysis.

However, in spite of Judge Frank's hope that the book will be useful for "intelligent non-lawyers as well as lawyers," it is doubtful whether it will have any special appeal to those without formal education in the

law. Max Radin's *The Law and Mr. Smith* is essentially the kind of book which most psychologists would find more useful in understanding in broad terms what the law is about. Frank's volume, on the other hand, is after all really written for lawyers—and, it might be said, only for that small percentage of lawyers who are essentially legal scholars. At least, only those persons with considerable legal training are likely to be interested in reading the volume in detail.

STEUART HENDERSON BRITT.

McCann-Erickson, Inc., New York City.

JOHNSON, PALMER O. *Statistical methods in research.* New York: Prentice-Hall, 1949. Pp. xvi+377. $5.00.

Several characteristics of this book stand out. The book is primarily an exposition of the methods, techniques, and approaches to statistical problems developed by R. A. Fisher and others. The purpose of the book is not to present a compilation of formulas and derivations. Few formulas are derived, but sources are cited so that the more advanced students may easily find the original papers. The real purpose seems to be that of presenting statistical techniques as research tools; showing how they are applicable, under what conditions each should be used, and permissible ways of interpreting the computed statistics. The emphasis of the book is on the interpretive rather than the descriptive function of statistics. "Thinking statistically is equivalent to thinking scientifically."

The book was written as a text book for a year course for students who have had some training in statistical techniques. While it is claimed the book was written for readers without *specialized* mathematical training, it must be noted that this does not imply "*without* mathematical training." For text book use, each chapter is well equipped with problems and examples to be solved. The examples used to illustrate the principles presented are drawn from real problems, and hence add to the realism of the concepts presented. The problems for the student to solve have more reality than is customary in statistical text books.

It is difficult to isolate any few chapters as more outstanding or better written than others. The chapters primarily concerned with the testing of statistical hypotheses will probably contribute more to the thinking of graduate students than do some of the other chapters. For other readers, some of the other chapters will be more valuable because of their "net" contribution to their thinking and to their techniques.

The book is valuable as a reference book, a source where one may find a variety of statistical functions with more than their mathematical expression—with information regarding the applicability and interpretation of such functions. Five useful tables are included at the end of the book. These are tables of normal distribution, t, χ^2, F, and L_1.

HAROLD A. EDGERTON.

Richardson, Bellows, Henry & Co., Inc.

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